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# JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH

## January Meeting, 1899

SUBJECTS: A Simple and Accurate Method of Carbon Dioxide Determination — The Branching Form of Bacillus Diphtheriæ as an Aid in Bacteriological Diagnosis — Rabies in Boston — The Refuse Disposal Plant of Boston

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# THE JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

The Massachusetts Association of Boards of Health was organized in Boston in March, 1890, with the following objects: the advancement of sanitary science in the Commonwealth of Massachusetts; the promotion of better organization and co-operation in the local Boards of Health; the uniform enforcement of sanitary laws and regulations; and the establishment of pleasant social relations among the members of the Association.

All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

The Official Journal of the Association is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them. No part of this matter is printed in any other periodical.

The Journal will present, from quarter to quarter, a fair and adequate picture of the progress of practical sanitary science as applied to the needs of a modern community. The various subjects which are reviewed in the quarterly meetings of the Association are treated by experts qualified to speak from daily experience in Public Health offices, who, as men of science, are careful to be scientific and comprehensive, and who, as public officers, are no less careful to speak pertinently and so as to be easily intelligible to the layman.

The Journal, in a word, appeals to all whose interests touch the questions of sanitation and hygiene,— to the architect, the school-committee-man, the manufacturer, the contractor, and, above all, to the busy practitioner who has no time for any reading but what is brief and to the point.

The subscription price of the Journal is one dollar a year, payable in advance. Single numbers, twenty-five cents. It is on sale at the Old Corner Bookstore, and at Smith & McCance's Bookstore, 57 Bromfield Street, Boston.

All communications to the Association should be addressed to the Secretary, **EDWIN FARNHAM, M.D., City Hall, Cambridge, Mass.**

Subscriptions and all business communications should be sent directly to the publishers,

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JOURNAL OF THE

# MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

*ORGANIZED 1890.*

[The Association as a body is not responsible for statements or opinions of any of its members.]

VOL. IX. March, 1899. No. 1.

## JANUARY MEETING

OF THE

## Massachusetts Association of Boards of Health.

THE January quarterly, or regular annual meeting, of the Massachusetts Association of Boards of Health was held in Boston, at the Parker House, on the afternoon of Thursday, January 26, Dr. Samuel H. Durgin, vice-President, in the chair.

THE CHAIRMAN.— If any one has a name to be submitted for membership in the Association, he should write it upon a card, and give it to a member of the Executive Committee. This committee will meet now at this end of the room to receive and act upon those names and any other business which may come before it. Names should be handed in at once.

HON. E. L. PILSBURY.— Might it not be a saving of time to appoint a committee to nominate officers while the Executive Committee is in session?

THE CHAIRMAN.— Very good. We should come to that in a few minutes, but perhaps it would be well to have the committee appointed at once.

HON. E. L. PILSBURY.— I move, Mr. Chairman, that a committee of three be appointed by the chair to serve in that capacity.

The motion was adopted.

The Chairman.— The chair will appoint Mr. Coffey, of Worcester, Dr. Gage, of Lowell, Mr. Pilsbury, of Boston.

After the meeting of the Executive Committee, the meeting of the Association was resumed.

The Chairman.— While the committee is out, and the Secretary also out at the telephone, we shall have to wait their return before we can proceed with the programme. We can, however, listen to the report of the Treasurer, Dr. J. B. Field, of Lowell.

Dr. Field then presented the following report: —

## TREASURER'S REPORT FOR 1898.

### Receipts.

| | |
|---|---|
| Balance from 1897 | $573.73 |
| Interest at savings-bank for two years | 37.46 |
| Annual assessments | 219.00 |
| | $830.19 |

### Expenditures.

| | |
|---|---|
| Postage and revenue stamps | $34.90 |
| Printing | 28.65 |
| Cigars and dinner for guests | 11.50 |
| Clerical assistance | 3.50 |
| Total expenditures | $78.55 |
| Balance to 1899 | 751.64 |
| | $830.19 |

Of this balance of $751.64 to next year, $537.46 is drawing interest.

Respectfully submitted,

JAMES B. FIELD, *Treasurer.*

Examined and approved as correctly cast and properly vouched.

J. Arthur Gage, *Auditor.*

The report was accepted, and placed on record.

The Chairman.— The next business is the reading of the records of the last meeting by the Secretary.

Dr. Farnham read the records of the meeting which was held at Lawrence, Oct. 27, 1898.

THE CHAIRMAN. — Are there any corrections to be made in the records? If not, they will stand as approved. The Secretary will read the report of the Executive Committee.

Dr. Farnham read the names of the following gentlemen, all of whom were elected members of the Association upon his motion: —

| | |
|---|---|
| THOMAS HUNT . . . . . . . . . | Cambridge. |
| RUSSELL S. NYE . . . . . . . . . | Falmouth. |
| FRANCIS P. DENNY, M.D. . . . . . . | Brookline. |
| P. F. LEARY, M.D. . . . . . . . . | Turner's Falls. |
| EMIL C. STIEGLER . . . . . . . . | Lawrence. |
| JOHN GILBERT . . . . . . . . . | Fall River. |
| HORACE E. MARION, M.D. . . . . . | Brighton. |
| DAVID W. TINSLEY . . . . . . . . | Fitchburg. |

THE CHAIRMAN. — There has been a vacancy in the committee for the supply of scientific papers by the resignation of Dr. McCollom, of Boston. The filling of this vacancy rests with the Executive Committee. That committee has appointed Dr. Curtis of Newton to fill the vacancy. We will next listen to the report of the Committee on Nominations.

HON. E. L. PILSBURY.— I will read this list of nominations, because the extreme modesty of my colleagues, who are both candidates for office, forbids their presenting the names: —

*President.*

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*Executive Committee* (for two years).

J. C. Coffey, Esq., of Worcester.
W. H. Chapin, M.D., of Springfield.
H. L. Chase, M.D., of Brookline.
J. A. Gage, M.D., of Lowell.
R. L. Newcomb, Esq., of Salem.

And there is a vacancy in the committee list which will expire in 1900, by the resignation of Nathaniel Hathaway, of New Bedford; and the name submitted is that of Thomas W. Cook, of New Bedford.

Respectfully submitted by the Committee.

The Chairman.— Gentlemen, you hear the report of the committee. What is your pleasure?

Colonel Morse.— I move that the Secretary be authorized to cast one ballot for the election of the officers named by the Nominating Committee.

The motion of Colonel Morse was adopted.

Dr. Farnham.— Mr. President, I hereby cast this ballot for officers of the Association.

The Chairman.— The officers named have been elected. Is there any miscellaneous business to come before the meeting? If not, we will proceed to the first paper on the programme, "A Ready and Accurate Method of Carbon Dioxide Determination, with Demonstration."

# A SIMPLE AND ACCURATE METHOD OF CARBON DIOXIDE DETERMINATION, WITH DEMONSTRATION.

BY G. W. FITZ, M.D., OF HARVARD UNIVERSITY.

Quantitative methods for the determination of carbon dioxide have long been sought,—formerly, because it was supposed to be the dangerous part of respired air; at the present time, because the amount produced by the processes of vital oxidation is thought to be directly proportional to the quantity of the volatile poisonous elements. The methods in use for its determination have been many. Pettenkofer's method, with recent modifications, is accepted as the most exact, except perhaps certain absorption methods that can only be carried out in a laboratory. The "ready" methods for its determination, however, have been fairly numerous.

Wolpert's method, used by the State police for the examination of school-houses, is one of the earliest. It depends, in principle, on the fact that lime-water becomes milky and opaque through the formation of calcium carbonate by union with the $CO_2$ of the air tested. A standard opacity is determined by noting when a black spot can no longer be seen through a certain depth of the milky solution. The method is quite inaccurate, varying with the rapidity with which the air is forced through the solution.

The younger Wolpert has devised a method, which is much more accurate, depending upon the neutralization of a standard alkaline solution (sodic carbonate), using phenolphthalein as the indicator. Wolpert has used this for testing the purity of air in contact with the body under the clothing, and for air in adjacent rooms as well as for the ordinary determinations. The method is accurate and convenient. The solution, however, must be made with care, since a varying amount of water of crystallization in the sodium carbonate makes serious variation in the strength of the solution. For ordinary school examinations the method is beyond the technical skill of the teachers, and hence unreliable.

In order to get a simpler method, I made some experiments with

lime-water, to determine how far a saturated solution would serve as a standard solution. I tested a number of saturated solutions from lime bought in different parts of the country, and found that the alkalinity of 100 c.c. was practically uniform in all. It was thus found possible to make a standard solution by merely measuring out a stated amount of a solution of lime-water saturated in the presence of an excess of the hydrate and at ordinary room-temperature.


After various experiments, I found that an apparatus such as I now present answered both for measuring the volume of the air used and for shaking the air and lime-water together. It is simply a cylinder or cup of glass, with rounded bottom, and a smaller open cylinder, with a neck resembling that of a bottle at one end. The smaller cylinder is made to slip into the larger through a collar of rubber tubing, so that it forms an air-tight piston. When the inner cylinder is drawn out, air enters through its mouth. The amount of air drawn in can be measured by means of the graduations on the side of the larger tube, the bottom of the inner tube serving as an index.

A 1 per cent. solution of a saturated solution of lime-water was found most serviceable for the tests, and may be made as follows: about 95 c.c.'s of water, to which several drops of the phenolphthalein solution have been added as an indicator, are neutralized by the addition, drop by drop, of lime-water with shaking. As long as the pink color caused by the lime-water

disappears upon shaking, the addition is to be continued; but, when a faint tinge of pink remains, we may consider that all the carbon dioxide in the water has been neutralized. By the addition of 1 c.c. of lime-water and enough water to fill the flask to the 100 c.c. mark, a standard 1 per cent. solution is formed, since none of the 1 c.c. of the lime-water is neutralized or lost. 10 c.c.'s of this solution are taken out by means of a 10 c.c. pipette, and put into a 3-dram homœopathic vial. This solution holds its strength for about twelve hours, and, when put into vials, can be carried anywhere in one's pocket. The whole operation of making up the solution requires five to ten minutes, and is exceedingly simple. The only apparatus necessary is a 1 c.c. pipette, a 10 c.c. pipette, and a 100 c.c. flask.

To make a $CO_2$ determination, the contents of one vial are poured into the testing apparatus. The inner cylinder is introduced, pushed down to the bottom, and then raised to the tare mark on the outer glass. This gives 30 c.c.'s of air for the first shaking. The right index finger is closed over the mouth, and it is shaken vigorously 30 times. If the color remains, the inner cylinder is run down to the bottom and drawn up to the 20 c.c. mark, giving 30 c.c.'s + 20 c.c.= 50 c.c.'s of air. This is repeated, and an account is kept of the amount of air used until the point of decolorization is reached. When great accuracy is required, the first test may be assumed to be experimental, and the test repeated with careful additions of small amounts of air toward the end.

As soon as the amount of air has been determined, the number of parts of $CO_2$ in 10,000 can be read off in the table, and the whole operation is complete.

TABLE.

| *Air in c.c. used.* | $CO_2$ *in* 10,000. | | *Air in c.c. used.* | $CO_2$ *in* 10,000. | |
|---|---|---|---|---|---|
| 30 | 28 | Very bad. | 103 | 8 | Fair. |
| 36 | 22 | | 117 | 7 | |
| 46 | 18 | | | | |
| | | | 138 | 6 | Good. |
| 58 | 14 | Bad. | 165 | 5 | |
| 69 | 12 | | 207 | 4 | |
| 82 | 10 | | | | |
| 91 | 9 | | | | |

When a large number of observations is to be made, it is simpler to take bottles holding 100 c.c.'s of the solution and a 10 c.c. pipette by means of which the amount necessary for each test can be transferred directly to the apparatus.

A comparison of the method with that of Pettenkofer shows that it is practically exact. I think I can say that, in the hands of an ordinarily careful man, it is exact within one part of $CO_2$ in 10,000 of air, which is a greater degree of accuracy than is required for ventilation determinations.

The testing solution will not keep its strength longer than one day. Various methods* for preserving the solution have been tried without success. If the bottles and stoppers are boiled, and sterilized water is used for making up the solution, it may retain its strength fairly well for two or three days. This is, however, so uncertain, and the solution is so readily made, that the question of preservation is practically unimportant. In ten minutes one can make up enough solution for a full day's tests.

I will pass the apparatus about.

The Chairman.— Any remarks to be made upon Dr. Fitz's paper?

Dr. Smith.— I should like to ask Dr. Fitz what causes the deterioration of the solutions. Precipitation of the lime carbonate?

Dr. Fitz.— It seems to me to be due to the evolution of $CO_2$ in the water and the consequent change of the hydrate of lime to the carbonate, doubtless through bacterial activity.

Dr. Smith.— Lime-water is antiseptic.

Dr. Fitz.— Saturated lime-water will keep perfectly well, but this is only a 1 per cent. solution.

The Chairman.— Professor Kinnicut, of Worcester.

Professor Kinnicut.— I can add very little to the paper that we have had the pleasure of hearing from Dr. Fitz. Certainly, we owe to him a very easy and very accurate method of determining carbon dioxide in the air. I have used Dr. Fitz's method in my own laboratory, in the Polytechnic Institute in Worcester, and have had

* Dr. M. H. Bailey made a large number of experiments with this in view, but found nothing really satisfactory.

my students use it during the last six months, and I find that analyses made by Dr. Fitz's method agreed very closely with analyses made by Dr. Pettenkofer's method, which, as Dr. Fitz says, is a method that can only be carried out by a man trained in analytical work and where he has a chemical laboratory at his disposal. The results of the two methods agree to within one part in ten thousand, and with careful work they agree even closer. For instance, during the past month one of the students of the Senior Class has been studying the ventilation of the Worcester Public Library; and I find in looking over his work the following results: —

One series of analyses gave 7.15 parts of carbon dioxide in 10,000 parts of air by Hesse's method, a modification of Pettenkofer's method, and 7.3 by Dr. Fitz's. Another series gave 12 parts by Hesse's and 12.8 by Fitz's. A third series, 6.3 parts by Hesse's and 7 by Fitz. These results show that Fitz's method gives a little higher result than that obtained by Hesse's, and I have found this to be the usual rule; but, the difference not being more than 1 part in 10,000, the variance is of small account, when studying the ventilation of rooms or halls. By using Dr. Fitz's method a large number of analyses can be made during the morning session of a school. One man can make 50 to 75 determinations in a morning, and the results compare most favorably with those obtained by Pettenkofer's or Hesse's method, where each determination takes at least one-half hour. Working in this way by Dr. Fitz's method, the effect of airing the room by opening the windows, and the sudden change that takes place in the air of the room during a recess, is very prettily shown. An interesting study can also be made with Dr. Fitz's apparatus of the effect of burning kerosene oil. I have often had my students take the apparatus to their rooms, and study the effect of a burning study-lamp on the air of the room. The amount of carbon dioxide in the air at the beginning of the evening being between 5 and 6 parts in 10,000, the student will often find that it reaches 22 to 25 parts at the end of the evening. This is not to be wondered at, as one student lamp will give off about 3 times as much $CO_2$ per hour as one person will exhale in that time. I suppose that Dr. Fitz will agree with me that really these 22 parts —

most of it coming from the lamp, and not from the breath — give no trouble or any disagreeable symptoms. In this connection there is one thing we should remember; and that is that carbon dioxide is not in itself, in any quantity that is found in a room, poisonous. In fact, work can be done easily in air that contains 1 per cent. of this gas. Carbon dioxide, as it occurs in the air of a room, is only a comparative measure of certain poisonous substances given off in the breath or from the person, whose exact nature we do not know, and is also only a measure of the impurities when the carbon dioxide comes from the breath, and not from lamps or gas.

I have also had my students using Dr. Fitz's apparatus determine roughly the amount of fresh air that enters a room. If the air of the street contains 5 parts of carbon dioxide in 10,000, and if each person breathes out 22 litres of this gas per hour, to keep the air of a room so that the carbon dioxide shall not be above 7 parts in 10,000, which is now required by the best sanitarians, 3,000 cubic feet of fresh air must enter the room per hour for each person present. Consequently, by counting the number of persons present and determining the carbon dioxide each hour, we can say how much fresh air is entering the room. If we find that the carbon dioxide is 8 parts at the end of an hour, only 2,000 cubic feet per person is entering the room each hour. If 10, only 1,200 cubic feet. In conclusion, I think we all feel we owe a debt of gratitude to Dr. Fitz for working out so carefully a process which cannot help being of great use in enforcing better ventilation in our public schools.

Dr. Abbott.— Mr. Chairman, I should like to say just a word about the practical importance of this matter. The Board of Health of Boston has made a wonderful advance in the medical inspection of schools; but it looks mainly upon one side,— that is, the infectious disease side,— while the subject of ventilation has been placed by law in charge of the district police. About a dozen years ago a letter came to the office of the State Board of Health from the School Committee of a small town, asking how they could determine the quality of the air of a school-room. In looking about, I found this description of Wolpert's instrument in one of his papers, and had this apparatus made. I would like to show just where its defects are.

When the district police came to me, and asked where they could get this apparatus, I told them by no means to rely upon it, since it gave only approximate results.

The defects are these: First, there is a rubber bulb; and every one knows that a rubber bulb goes to pieces in a short time, and becomes worthless. For instance, this one, which has been lying unused for several years, is so stiff that it is useless. Second, it is not accurate. One man will squeeze it very tight, perhaps with both thumbs, another one with only one thumb; and you do not eject all the contents of the bulb. Hence the sample of the air of the room is not an equable sample, under all circumstances, in quantity. Third, when it becomes old, there is an escape of air at the joint where the glass tube enters the bulb. I find this one allows half the air, at least, to go out at the joint instead of through the opening at the end of the tube. There are quite a number of just such defects as I have described. Here is another, in the tube containing the lime-water. You have to rely upon the vision. The observer must depend upon his eyesight to inform him when the little black mark at the bottom becomes sufficiently obscured so that he cannot see it. So there are quite a number of defects that account for its unreliability. It seems to me that anything that is more reliable deserves consideration.

Professor Rietschel, of Berlin, has done some excellent work in this line, and published some years ago a report of his examination of the schools of that city,* with diagrams showing just this point that Professor Kinnicut has illustrated; that is, the increase in the amount of carbonic acid, and then the sudden drop at recess, and then the increase again afterward, and so on. This certainly is a very useful instrument; and it seems to me that something of the kind, such as has been devised by Professor Fitz, ought to be in use in all our schools, and not only in our schools, but in all public buildings where people congregate in large numbers.

Dr. Davenport. — I was not in early enough to hear the beginning of Dr. Fitz's paper. I should like to inquire, therefore, if he pays any attention to the temperature of the air at the time of making the experiments. If he does not, how are his results comparable

* Lüftung und Heizung von Schulen, Berlin, 1886.

when taking air at the different temperatures between 32° and 70° Fahrenheit?

A MEMBER.— Does the temperature of the air affect the result?

DR. FITZ.— I found that the ordinary range of temperature in rooms made no difference whatever, and I have reason to believe from my experiments that any temperature above freezing does not vititate the results. But, of course, with considerable ranges of temperature an allowance must be made for the density of the air, as is done in Pettenkofer's and Hesse's methods.

DR. BROWN.— Mr. Chairman, I should like to ask by whom this apparatus is manufactured, and where it can be had, and how much it costs, and if with it there are printed directions that will enable the purchaser to make the tests or learn how to make them.

DR. FITZ.— Mr. Chairman, the apparatus is made by the Knott Apparatus Company, 16 Ashburton Place, near the State House, Boston; and complete printed directions go with it. It costs two dollars ($2.00), including the case and pipettes.

THE CHAIRMAN.— It is strikingly interesting to listen to the paper of Dr. Fitz, especially as he has brought forth a very practical as well as an exact means for determining the amount of carbon dioxide in the atmosphere. I cannot refrain from asking Dr. Fitz, however, if by means of this first-rate method he has found any condition in the schools of Cambridge yet which has awakened any interest on the part of the city authorities [laughter]. Are there any further remarks to be made upon Dr. Fitz's paper?

DR. GAGE.— I should like to ask if it is necessary for a chemist to prepare this solution every morning, or can each individual prepare it for his own use under the directions?

DR. FITZ.— Any one can make the solution. It requires no skill.

THE CHAIRMAN.— The next paper on the programme is one "On Branching Forms of Diphtheria Bacilli," by Dr. H. W. Hill, of Boston. Dr. Hill has been affected in the last twenty-four hours by the prevailing epidemic, and is not able to read his paper; and he has asked Dr. Brough, of Boston, to read it for him.

Dr. Brough then read Dr. Hill's paper "On Branching Forms of Diphtheria Bacilli."

# THE BRANCHING FORM OF BACILLUS DIPHTHERIÆ AS AN AID IN BACTERIOLOGICAL DIAGNOSIS.*

BY HIBBERT WINSLOW HILL, DIRECTOR.

The published articles of nineteen or twenty investigators, dating from the year 1887 to the present time, have established the fact that the bacillus tuberculosis is often found to branch. The bacillus of diphtheria has also shown a similar peculiarity. It has long been held by bacteriologists that bacilli, properly so called, do not branch. To make this subject clear, it is necessary to recall to you five principal classes of bacteria,— the cocci, the bacilli, the spirilla, the cladothricheæ and the streptothricheæ. The cocci are spherical in shape. The other four classes consist of individuals all of which are more or less elongated, forming rods or threads. The bacilli consist of those rods which lie entirely in one plane. They may be bent or curved, but are never twisted spirally. The spirilla, however, occupy more than one plane; for they are not only bent or curved, but also twisted so that they appear like a spiral spring or a cork-screw. These two classes, the spirilla and the bacilli, do not show branching when they divide, the two new rods thus formed often remain lying end to end, united by some intercellular substance, and long chains may thus result. Amongst the cladothricheæ, which are also elongated thread-like forms, the distinguishing feature is the fact that, when division takes place, the two rods thus produced do not separate from each other, nor do they remain lying end to end. The two new rods remain united by a small amount of intercellular substance, it is true; but, instead of lying end to end, one rod grows at the point of division, pushes a little to one side and elongates past the other rod. The second rod then seems to be a branch of the first rod, which now appears as the main stem. It is nevertheless quite evident that the two rods are independent of each other. In the streptothricheæ, the last of these classes and the highest form of the bacteria, true branching occurs. A little protuberance pushes itself out from the side of a rod, and then elon-

* From the laboratory of the Boston Board of Health.

gates more or less until the two rods lying together present a sort of an irregular T. Sometimes one rod will give rise to two or more branches, or a branch may give rise to another branch of its own. In this way somewhat complicated figures may result.

The extreme irregularity of outline found in the rod-shaped organisms which are called the bacilli of diphtheria is one of the main features by which the bacteriologist recognizes them under the microscope. The diphtheria bacillus is distinguished from almost every other known bacillus by this very irregularity. All bacteriologists know that, when, toward the end of a diphtheria case, the cultures show a diphtheria bacillus fairly regular in shape, the difficulty in recognizing it at once increases. If the bacillus of diphtheria were regular in outline, we could have no such diphtheria diagnosis service as we have at present; for the diphtheria bacillus would under such circumstances resemble other bacilli so closely that no one could differentiate them microscopically. It will be seen, then, that the irregular shape of this bacillus is of great practical importance. The question naturally arises, Why is this bacillus almost the only one which presents these irregularities? Generally, bacteriologists have been satisfied to accept the fact without offering a satisfactory explanation. It cannot, however, be merely a provision of nature to enable boards of health to diagnose the disease. Without going into the matter more deeply at the present time, I have ventured to record my belief that the explanation why the diphtheria bacillus differs from all other bacilli lies in the fact that it is not a bacillus at all. This is a simple explanation, and yet an explanation which is somewhat in advance of generally accepted ideas. Nevertheless, as already stated, this belief has been growing for some time among those bacteriologists who have paid special attention to this subject.

If, then, this bacillus is removed from the class of bacilli and placed among the streptothricheæ, or cladothricheæ, what practical advantage is gained? Of course, scientifically, it is clear that it should be placed among, say, the streptothricheæ, if it belongs to that class, even if nothing else is gained than a greater accuracy in classification.

In addition to this, however, such a change in classification would

simplify matters somewhat. It is much more easy to understand the curious forms of this organism which are found under the microscope, if they are considered as isolated portions of a branching form,— that is to say, degenerated, broken-down parts of a larger and higher plant,— than to wrench our ideas of the typical bacillus to suit the peculiarities of this one species. To consider the diphtheria organism a bacillus forces us to all sorts of extremities in trying to account for its swollen ends, its peculiar granules, and the branching forms which it sometimes presents. To consider it a member of a higher group accounts for all these peculiarities at once without straining our imaginations or credulity.

There is, however, a third and more practical view of the situation. Whether it is agreed to call the diphtheria organism a streptothrix or not, the fact remains that branching forms of this organism may be found in considerable percentage of the cultures taken for the diphtheria diagnosis. This is my own experience, and, as I have found on inquiring, the experience of others who have done much of this work. It is true that hitherto little regard has been paid to the subject. Since I began watching for these branching bacilli, I have been struck by the help in diagnosis sometimes given in doubtful cases by finding in the preparation under the microscope one or more of the branching forms. Every bacteriologist who has examined cultures for diphtheria diagnosis knows that it is often possible to state definitely, after a very short examination, that a given preparation does show or does not show the presence of the diphtheria bacilli. Every bacteriologist knows also, however, that in some cases a very careful and laborious microscopic search will still leave him in a doubtful frame of mind as to whether or not the organism he finds is really the diphtheria bacillus or not. This is especially true in those cases referred to, where the diphtheria organism is more regular in shape than usual, and therefore resembles more closely other bacilli. In such cases the finding of a branched form of the proper size and staining reaction may be of some considerable help. If a branched form cannot be found in a given doubtful case, the bacteriologist is no worse off than he would be if he did not pay any attention to the existence of branched forms at all. If a branched form is found, however, he has one more point on which to base his

diagnosis. I do not wish to give the impression that every branching organism which may be found in a culture from the throat is necessarily a diphtheria bacillus. On the contrary, I wish to make very clear the fact that branching forms of another organism do also occur. This other organism will, however, give little trouble, for it consists of very long and very wide rods and threads, it stains very uniformly and presents no granules nor unstained spaces; while, on the other hand, the branching diphtheria bacillus is made up of sections each of which is by itself not decidedly larger than an ordinary unbranched bacillus, and each section shows generally unstained spaces, granules, and swollen ends also like those of an ordinary bacillus. In those cultures where the sections of a branched diphtheria bacillus are regular in outline, the smaller size will still permit its differentiation from this other and larger organism.

Dr. Brough.— Dr. Hill has some illustrations of this organism on the door at the end of the room which he wishes to show.

Dr. Hill then described the illustrations briefly.

The Chairman.— Any remarks to be made upon Dr. Hill's paper?

Dr. Connell.— Mr. President, I enjoyed Dr. Hill's paper very much. I appreciate also the difficulty that he has expressed in making a positive diagnosis of these diphtheria bacilli. It seems to me particularly difficult in the course of convalescence after local applications of antiseptics have been made. At such times and under such circumstances I have found that the diphtheria bacilli undergo changes, and consequently the difficulty of determining their true nature increases. Then, too, as a result of this, the question of deciding whether or not the house can be relieved of quarantine is harder to decide. The difficulty that I have found about the branching forms, as he suggested, too, is that they are so few in number compared to other forms that you cannot say positively just what their relation to the single rods may be. His paper, I think, is helpful; and, if we could find them more abundantly in our cultures, the help would be more beneficial than it is now. At any rate, the subject is one that is extremely interesting, and, I think, will be helpful to us in making positive diagnoses.

The Chairman.— Shall we hear from Professor Smith?

Professor Smith.— Mr. Chairman, I wish simply to emphasize the statements just made; namely, that the subject is a very interesting one, and, while it may be at present of but very slight practical importance in making diagnoses, it may lead us after a time to determine the origin and source of the diphtheria bacillus,— in other words, to trace it back to growths which are like it, and which possibly may help us to know something about the origin of the disease itself. Of course, this is a purely academic question; but still I think it interests us all. The forms that are presented there resemble in many respects those of actinomycosis and certain forms of the tubercle bacillus. These and the diphtheria bacillus may be found to date back to a common ancestry before they were disease-producing germs at all. The question, in this form, is of great interest to the biologist.

The Chairman.—Any other remarks upon Dr. Hill's paper? If not, we will proceed to the next paper on the programme, "Rabies in Boston," by Dr. Alexander Burr.

## RABIES IN BOSTON.

Although rabies is a comparatively rare disease, it presents such distressing symptoms when it does occur that universal attention is at once directed to it. The recent occurrence of several cases in Boston makes the subject of more or less interest at this time. Rabies is an acute febrile disease, having a varied period of incubation. It is found in all warm-blooded animals, but is more especially known to us among dogs, occasionally being found in horses, cattle, and other domestic animals. It is a well-recognized fact now that rabies is caused by the inoculation, directly or indirectly, of the virus from animals affected with the disease. Directly, as when a person or animal is bitten by a rabid animal, the virus being carried through the skin by the teeth of the rabid animal. Indirectly, as in the case of an animal licking a sore on an animal in the early stages

of the rabies; licking of posts, etc., which have become smeared with the saliva from a rabid animal; also the occupation by animals of kennels that have been previously occupied by rabid animals.

Man becomes infected by the bite of a rabid animal or from the scratch by the claws of a rabid animal which has previously licked its claws.

Bites upon the exposed parts, as the hands and face, are more fatal than those through the clothing, as the clothing tends to clean the teeth of the saliva before reaching the skin.

Spontaneous cases of rabies are now known to be impossible. It is accepted by all authorities that the virulent principle is a living germ; but, unfortunately, all attempts thus far to isolate and distinguish it have failed. This virulent principle is found in the tissues of the brain, spinal cord, saliva, and other fluids of the body. The injection of a very small part of the brain or saliva of a rabid animal produces a fatal general disorder. Inoculation from one animal to the other may be carried on indefinitely. This, together with the fact that the period of incubation is more or less prolonged, would tend to show conclusively that rabies is not the result of poisoning by some chemical agent, but that it is due to a living germ that propagates when injected into an animal. Rabies agrees with all other germ diseases in that it develops after inoculation, and that the attack protects usually against the second. At first it is confined to the region of the bite; but, when fully developed, it is found in all vascular tissues.

Period of incubation varies from a few days to months, the average period in dogs being about twenty-five days. In man the period of incubation is extremely variable. In 6 per cent. of the cases, it is between three and eighteen days; 64 per cent., between eighteen and sixty-four days; 34 per cent., exceeds sixty days. From fourteen days to two or three months is the limit.

*Symptoms in Dogs.*

For the first two or three days the symptoms are similar in all forms of rabies. There is noticed considerable change in the habits of the animal, restlessness or sullenness. In other cases, the animal

becomes more affectionate, and liable to infect by licking the hands and face of persons handling it. There may be dulness, but more frequently hyperæsthesia. The dog starts quickly at the slightest noise; has a tendency to pick up and swallow foreign bodies, as bits of wood, stone, dirt, cloth, etc. Sexual excitement is marked; sudden passion is developed when another dog is seen. The animal secludes itself in dark places. It barks and howls, and starts nervously without any apparent cause.

The bark is quite characteristic, in that it is a low, hoarse, muffled sound, beginning as a bark and prolonged into a howl, which may be repeated two or three times without closing the mouth. Following these primary symptoms, the case generally develops into one of two forms, — the furious (also called mad form) or the paralytic (also called dumb form).

The furious form is shown by paroxysms of violence, during which the animal generally escapes and runs away for many miles, attacking and biting anything in its way. It seldom stops for any long fight with another dog, but makes a quick bite, and is off again. It may or may not return home after the paroxysm has passed off. Between the paroxysms, the dog shows symptoms similar to those during the first two or three days. The paroxysms are repeated, and in two or three days there is loss of power of the hind legs, which is the beginning of general paralysis. This is followed by complete paralysis of the hind legs, which results in the animal being unable to move about. This is followed by partial and then by complete paralysis of the front legs. Dropping of the jaw, paralysis of the pharynx, and frothing from the mouth follow. Gradually complete paralysis sets in all over the body, and death results, generally within ten days of the first symptoms.

In the paralytic or dumb form of rabies the paralysis follows the primary symptoms at once, and is similar to the later stages of the furious form.

*Diagnosis.*

In dogs the diagnosis is attended with more or less difficulty. One is seldom called until the later stages, often not until the animal has been destroyed. Street cases are attended by all sorts of wild

stories; and it is almost impossible to obtain a clear history, especially of a previous bite. With the symptoms as described and a history of bite, the diagnosis is fairly clear. The statements of the owner or person in charge are often very misleading. For instance, the dropping of the jaw is invariably described by the owner as due to a fish-bone, chicken-bone, or some other foreign body in the throat. It is exceedingly difficult to make the owner change his mind in regard to this; and so firmly is his statement adhered to that it often results in the veterinarian making an examination of the mouth, and thus running the risk of a bite or scratch from the teeth of the animal on the hand.

An absolute clinical diagnosis cannot be made; but often very suspicious symptoms are shown, which justify further examination.

*Post-mortem* examination shows absolutely no characteristic lesions either to the naked eye or the microscope. Generally, the stomach contains foreign bodies; and these foreign bodies, in the absence of pathological changes, may be looked upon as pointing strongly to rabies.

The only method of absolute diagnosis is by injecting a small portion of the brain or cord of a suspected animal into rabbits, and is done as follows: The brain and upper portion of the spinal cord of the suspected animal are removed as aseptically as possible. About one gram of this is crushed in a mortar, about 10 c.c. of sterilized water is added, and an emulsion made. This is thoroughly mixed, and filtered through a sterilized paper filter; and ½ c.c. of the filtered emulsion is injected into the subdural space. The rabbit is etherized, and a longitudinal incision made in the skin of the forehead just back of the eye. The periosteum is reflected. A trephine is introduced, and a small piece of bone is removed, and the dura exposed. The dura is punctured by the syringe needle, and the emulsion thus injected into the subdural space. The rabbit soon recovers from the ether, and eventually goes to eating, having suffered nothing from the operation.

The first symptom noticed in the rabbit is hyperæsthesia, the slightest touch causing it to start quickly. Following this there is paralysis of the hind legs, extending to the fore legs, and finally complete paralysis, followed by death. The period of incubation

in rabbits varies considerably, but the average time of death is about the fifteenth day. In the first rabbit inoculated, death may come on in thirteen days or may be delayed as long as three months.

*Post-mortem* examination on the rabbit also is very unsatisfactory. There is nothing to see, and the chief value of an examination seems to be to eliminate all other causes of death.

This outlines briefly the symptoms shown and the only method of diagnosis of rabies known at the present time. Rabies has been comparatively rare in Boston, but a sufficient number of cases has occurred within the last two years to demonstrate that very prompt and stringent measures should be resorted to at once. In 1896 two dogs were reported to the Board of Health as suffering from rabies. One of these on examination proved not to be a case. The other suspected case developed about December 15; and the dog was led to one of the veterinary hospitals of this city, where it died in about twelve hours. I am unable to say whether a diagnosis was made at that time or not; but a rabbit was inoculated from the dog at the Harvard Medical school, and a positive diagnosis of rabies made. The case was reported to the Board of Health on December 31, sixteen days having elapsed without any measures being taken toward preventing the spread of the disease or for the protection of the public.

Jan. 1, 1897, an investigation was begun by calling on the owner of the dog; and, to our surprise, it was found that he was also the owner of two other dogs both of which had been constantly with the rabid dog. On examination of these two dogs at this time, one was found to show very marked symptoms of rabies, and the other presented no symptoms. It was advised that both dogs be sent at once to the hospital as a protection to the occupants of the house and to prevent a possible escape of the dogs into the street. These dogs were followed to the hospital, and a quarantine placed upon them there. The first of the two dogs died at the hospital on January 5 with marked symptoms of furious rabies. The second, although in constant contact with the first dog until quarantine was established, showed no symptoms; and on January 9 the owner, desiring to prevent further expense, was allowed to take the dog home, to be there placed in safe quarantine for ninety days. This quarantine was

faithfully observed; and at the end of ninety days the dog, having shown no symptoms, was released. As late as June 30 an examination was made of this dog, and it was found in good condition.

At the time of the investigation on January 1 it was learned that the first of these three dogs, while being led through Boylston Street some time during the first of December, was pounced upon and bitten by a St. Bernard dog; and thus there was a clear history of bite. No trace of this St. Bernard dog was ever found; but it is probable that it ran off, and died somewhere within the usual time of ten days, long before the case of the dog bitten by it was reported to the Board of Health.

Here is a good example, showing the urgent need of reporting early such suspicious cases. Thirty days elapsed from the time of the bite and fifteen days from the time of death before the proper authorities had any knowledge of either,— ample time to have caused a wide-spread epidemic of rabies in this city; for at the same time there were two more dogs at the owner's premises, one of which had rabies, and was liable to escape at any time. Fortunately, these dogs were of very fine breed, and consequently were never allowed off the premises of the owner except on a leash.

In this case, all feeding-pans, dog-bedding, dog-boxes, etc., used were destroyed, and the room washed with corrosive sublimate.

Jan. 11, 1897, two more dogs were reported rabid in the same district. Although it was impossible to connect these cases directly with the St. Bernard dog, it was possible to exclude the last three dogs as causes. If these two cases were caused by the St. Bernard dog, it is probable that they were not bitten later than December 5; and there would then have elapsed thirty-six days from the time of bite by the St. Bernard dog. The owners of these two dogs were advised to kill at once, which was done.

During the summer of 1897 a number of dogs were bitten by a St. Bernard dog in Dorchester. This St. Bernard dog disappeared, and no trace of it has been found. All dogs known to have been bitten by this dog were killed by the owners or died at different hospitals.

On Dec. 1, 1898, a dog owned in West Roxbury was reported to the Board of Health as a case of rabies by the attending veterinarian,

who also killed the dog. The head of the dog was obtained, and sent to the Board of Health laboratory for examination. This was the first case which occurred after this laboratory was available for the purpose of examination and diagnosis of suspected cases of rabies.

A rabbit was inoculated in the usual way by Dr. Hill and myself on December 5. The rabbit is alive and well to-day, and has shown no symptoms of rabies. The dog was known to have been bitten by an unknown dog on September 15; but no symptoms were shown until Nov. 27, 1898.

There has been recently in Boston an outbreak of rabies which derives considerable interest from the following points: —

No history of the original dog having been bitten could be obtained. The affected dog had short repeated paroxysms, during which it would attack everything in its way. These paroxysms lasted for so short a time that the dog did not leave the district, but returned home after each. A great many dogs were either bitten or exposed to infection from this dog. A small boy was also bitten by the dog during one of its paroxysms. The danger from this outbreak was looked upon very lightly by a majority of the dog-owners. The conditions of quarantine maintained on the owners' premises were such that, while the quarantine protected the public to a certain degree, it afforded no protection to the owner or any one else who might visit the premises.

It is doubtful if this case would have come to the knowledge of the Board of Health, except for the fact that a boy had been bitten by the rabid dog. Saturday, Dec. 3, 1898, this boy, about twelve years of age, was bitten on the hand by this dog, and a slight wound inflicted. The owner of the dog joined the parent of the child in taking the child to Dr. Green, of Dorchester, who dressed the wound. Dr. Green requested the owner not to kill the dog, but to report to him at once if it should show any suspicious symptoms. On the following Wednesday, December 7, the owner reported to Dr. Green that the dog was acting in a suspicious manner, and was advised to have a veterinarian see the dog at once. The veterinarian, after hearing the statement of the owner, reported the case to the Board of Health immediately. I examined the dog at once at the owner's

house, and found it paralyzed on the kitchen floor, biting and snapping at everything within reach. It was immediately placed in a large box, chained up, and boarded in to prevent any handling by the occupants of the house. As was expected, the dog was found dead the following morning. A second dog owned by the same person, and known to have been bitten by this dog, and at the time of the first examination found playing with a small child in the house, was at once killed. A great many conflicting stories were obtained at this time from the owners and outsiders, the owners being more suspicious of rabies than the outsiders, who made light of the whole affair. A diagnosis of rabies was made, and the case treated as such.

Here was a case that called for prompt action. A child had been bitten, and it would have been criminal to neglect it. On consultation with Dr. Green the parent of the child was informed of the possible danger, and advised to send the child to the New York Pasteur Institute for treatment. This was done within a week.

All other dogs in this vicinity known to have been bitten or exposed to this rabid dog were placed in quarantine upon the owners' premises. All of the owners of suspicious dogs acquiesced in this quarantine, and made a very easy matter of it.

Although this quarantine on the owners' premises has been well observed, it was seen early in the outbreak that at the best it was a very poor method of quarantine; and, as a result, the Board of Health at once directed the erection of a detention hospital for dogs, to which all dogs not properly quarantined could be removed, and where their safe isolation would be insured.

Just a word in regard to this hospital. With the consent of the Election Department, a wooden polling booth was transferred to the Epidemic Hospital grounds on Swett Street, and a number of kennels at once put up in it. In the rear of each kennel is an independent yard. The partitions between the yards are of heavy wire, and all separated from each other by a four-inch space, thus preventing the possibility of dogs reaching each other through the meshwork.

On December 9 an autopsy was made by Dr. Hill and myself at the laboratory on the rabid dog, at which no pathological changes

were found. In the stomach was found a large mass of cloth, which, in the absence of pathological changes, was enough to warrant suspicions of rabies. A small portion of the brain and cord of the dog was injected into the subdural space of a rabbit in the usual way. On the eleventh day the first symptom, that of screeching, was observed, together with partial paralysis of the hind legs. On the twelfth day distinct attempts at screeching were noted; but the attempts were without success, probably due to the fact that there was paralysis of the larynx. Paralysis had extended to all four legs. Death resulted on the thirteenth day. An autopsy made at once showed no pathological lesions.

One of the dogs bitten by this one became rabid, and was killed by the owner. Seven other dogs were at once killed as a precaution also by the owners, and sixteen are now in quarantine. This outbreak has demonstrated to this department the necessity for such action as the following recommendations outline: —

1. There should be an improvement in reporting cases, for it is believed that many never reach the proper authorities.

2. The early report of all suspected cases. I think there is a tendency on the part of some to hesitate in reporting these cases for fear of an error in diagnosis. It hardly seems as though there could be any harm come by reporting these cases early, no matter how slight they may be.

3. The compulsory reporting by police, owners, and veterinarians of all dogs killed by them as dangerous animals. And in connection with this case I will just call your attention to the Melrose case, which you probably have all heard about, that of a dog which ran into a church on November 14, and bit a woman. That dog was killed within a few days, I don't know but it was the same day, by some one at the Melrose railroad station. Nothing was done or said about the dog at the time. It was not considered or thought of as a case of hydrophobia. The latter part of December or the first of January the woman died with hydrophobia. I mention this case to show that there was a possibility of treatment for that woman, if the case had been reported at the time the dog was killed. It was killed probably within twenty-four hours. A diagnosis could have been made then, within fifteen days, which would have been

before the last of November,— ample time for the woman to have gone on to New York for treatment, which would then have given her almost a month for treatment.

4. An autopsy should be made upon all dogs killed, if the symptoms are at all suspicious, particularly dogs killed on the street, the history in the latter cases being usually very unreliable.

5. The inoculation of rabbits for diagnosis from all cases reported to the Board of Health as rabid.

6. The strict quarantine for ninety days of all dogs bitten or exposed to infection from a rabid dog.

I think in this case the owners should be given the choice either of killing or quarantining.

The quarantine of the dog on the owner's premises should amount to more than preventing its escape. It should also insure the safety of the occupants of the house as well.

7. A detention hospital under the directions of the Board of Health, to which all dogs not properly quarantined at home may be removed. It would be convenient in connection with this if we had the authority to compel the removal and quarantine at such a hospital of all dogs which, upon investigation, had been exposed to infection.

8. All utensils used by the dog should be disinfected or burned, if necessary. The dog should be cremated. The walls and floors of the rooms in which it has been quarantined should be washed with corrosive sublimate, with a strength of about one to five hundred, or 5 per cent. carbolic acid solution. And in connection with this I would say that all corrosive sublimate solutions should be acidified or contain a small portion of common salt to prevent a precipitation of albumen: bichloride, 1; salt, 5; water, 1,000. Bichloride, 1; acid, 10; water, 1,000.

The Board of Health is now prepared to make inoculations on rabbits from all dogs suspected of rabies and to remove all such dogs to its detention hospital. It is also hoped that, with the improved laboratory facilities now at hand, it may do considerable experimental work in connection with the disease.

The Chairman.— Any remarks to be made upon Dr. Burr's paper?

Dr. Abbott.— I should like to inquire among those regulations whether there is no regulation in regard to muzzling dogs.

Dr. Burr.— Mr. Chairman, I purposely avoided going into that side of it. It had seemed to us that the muzzling of dogs is very unsatisfactory. The muzzle had to be removed from the dog at least twice a day for feeding, which of course was a risk, and also it is very frequent to find dogs in the street with muzzles off the head, hanging loose about the neck. While I think that in some cases there may be considerable benefit from the muzzling, a great many times it is of little value. While I do not recommend muzzling dogs all the time, as is done in some countries, there is no doubt but that, if all dogs were constantly muzzled in a proper way, rabies would be practically an unknown disease.

Dr. Miller.— Mr. President, I was very much interested in this paper,— an exceedingly scientific paper. I watched the doctor closely to see if he was not going to give us some remedies, some treatment. I really hope that he will be able to, or some gentleman present will. The only points I saw in that direction were three,— one was to quarantine the patient until it died, the other would be to kill the patient outright, and the third would be to send it to New York. I would like to know if the doctor can give us any remedy that we can apply immediately, or if any physician present can, some course of treatment which we can apply to our patients,— a course which we can have the patient follow at home.

Dr. Burr.— In regard to the treatment of rabid dogs, that, so far as I know, is not attempted. The treatment of the human patient has been carried on at many anti-rabic institutions with considerable success.

Dr. Miller.— The point I wanted to get at, Doctor, was whether or not there was any remedy that you could suggest that we could apply here,— that physicians could use, and keep the patient at home,— or if it would be better to send him immediately to the Pasteur Institute.

Dr. Burr.— So far as I know, there is no treatment for a human patient in this State, that is, there is no anti-rabic institution in this State.

Dr. Abbott.— Mr. Chairman, I should like to say one word

about this matter of muzzling. Muzzling dogs may not be an agreeable measure, in its practical application; but how is it in other countries? I once listened to a discussion in London on that subject. The question came up, and was introduced in the Congress of Hygiene, Shall Dogs be Muzzled? There was a vigorous opposition at once. The hunting privileges of the noblemen must not be interfered with. On the other hand, how is it in Berlin? No dogs without muzzles: no hydrophobia. Every man, woman, and child vaccinated at the age of one or two years and again at twelve years: no small-pox.* [Applause.]

A Member.— Mr. President.

The Chairman.— Dr. Gage, I believe, had the floor previously.

Dr. Gage.— I should like to say in beginning that, if I remember rightly, at one of the very first meetings of this Association we had two very interesting papers on this same question by Dr. Ernst and Dr. Peters. As I remember, one statement of Dr. Peters was that this disease suddenly appeared, and then by extension spread in the neighboring towns, and then disappeared, and after a certain period appeared again. I wondered if any observations had been made to determine any laws that governed that spread or the period at which it appeared, whether anything could be done in the way of forestalling any possible dangers from it, as, for instance, if the disease appeared in one town, whether notification might be sent to neighboring towns and to neighboring parts of the State, or any precautions taken. I will also put another question before I sit down, and leave the answers of both to come at once; and that is, the practical difficulties that the doctor has to contend with. A boy is bitten by a dog. I have two cases in mind that happened within a couple of years in my own practice. A man, intelligent, came to me, and said: "My boy has been bitten by a dog. They think the dog was mad.

* Examination of the records of England and Germany for the four years 1892–95, reveals the following facts: During these four years forty-three persons died in England of hydrophobia in a population, in round numbers, of thirty millions. During the same years, in Germany, in a population of fifty millions, only eight persons died of the same disease; and all of these eight cases without exception occurred in Königsberg, Silesia, Breslau, and other districts near the Russian and Austrian border, into which vagrant and rabid dogs may have strayed across the border from countries where dogs were not under strict control. So also with small-pox. Nearly all of the very few deaths from small-pox in Germany in recent years have occurred among immigrants from neighboring countries where vaccination is loosely conducted or is not compulsory.

They have killed the dog, and buried him out here in the country somewhere,"— he did not know exactly where, thought he could find out. "What would you do with the boy?" I did not know how long after death the dog's brain could be utilized for the experiment with a rabbit, but in a general way told him that, if possible, I should get that dog's head and have it sent to Boston at once, and then, if they found rabies, send the boy to New York. Unfortunately, it was not done; and the boy died with rabies. One of the selectmen of a neighboring town came to me, and said: "Doctor, what shall we do? A dog has bitten two boys, and we don't know whether the dog is mad or not." I believe in that case they lost track of the dog. The question that I had to answer was, Ought I to send those boys to the Pasteur Institute on the chances? I should like to say right here that my observation is that still a great many men in the profession take out their nitrate of silver, and send the family home comforted. I don't believe in that. I think the profession, the medical profession, would like to have the veterinarians lay down some rules which we can go by in giving our advice to our patients. Have I made my questions clear enough, so that the doctor can answer them?

Dr. Burr.— It seems to me, Mr. Chairman, that those questions are exceedingly hard to answer. It seems to me that the first attempt should always be to locate the dog; find the dog, if possible, and submit the dog to an examination,— that is, by an inoculation upon rabbits,— and in that way confirm the suspicion.

Dr. Gage.— After the dog has been killed, how long will its brain be serviceable if it has been buried?

Dr. Burr.— That would depend, it seems to me, entirely on the condition of the brain as we received it at the laboratory. If there was marked decomposition of the brain, I think it would be hardly worth while attempting to make the examination. At this time of year, of course, the head might keep in very good condition, and how long it would be impossible to say. If at once placed in an ice-box and frozen, I suppose it would keep for some time. I remember a paper by Dr. Frothingham, in which he spoke of the time that the emulsion might be kept after it was made from the brain. He had used an emulsion for injecting as old as a year and ten months, I

believe, which had been frozen at a very low temperature all this time, possibly at a lower than 32°.

Dr. Gage.—If the cases were suspicious, but you had not a positive diagnosis of the brain, if the presumptive evidence was in favor of rabies from the history that you could get, would you recommend that we should tell our patients to go to New York?

Dr. Burr.—A case that occurred in Boston was somewhat similar. The dog, while presenting symptoms of rabies, was almost in the last stages of it; that is, it died probably within twelve hours from the time it was seen. There was no history whatever of bite in this dog. I tried the best I knew how to trace it back, but it was impossible. The dog was presenting some symptoms similar to rabies, and a child had been bitten. In consultation with the parent of the child and the attending physician, it was thought best to let the parent know, and state to him just the dangers of the case; that his child had been bitten, and, while we could not say positively that it was a rabid dog, it was impossible to determine for fifteen days, and it did not seem as though it was wise to wait for fifteen days. With this knowledge the parent was to decide for himself. In his case, although a man working for $2 a day, he decided to go to New York at once; and I have no doubt that the total expense was about $300.

Dr. Parker.—Mr. President and gentlemen, the great trouble that the Cattle Commissioners have found in locating cases of rabies is due to the difficulty in getting the owners to report any such cases. No one ever reports until a number of dogs have been bitten. Another trouble, even after positive evidence of rabies has been found, is the difficulty in getting the local authorities to order the muzzling of dogs.

An interesting case occurred in Ipswich a little while ago that illustrates this point. A dog on the sixth day of December went rabid. It bit several other dogs, and among the dogs which were bitten was one belonging to a Mr. Kinsman. This dog was traced to the woods round Hamilton, and finally was shot in the woods and left there. Nothing was done about it, and no report was made of it until on December 21, just about a fortnight after he was bit, the dog belonging to Mr. Kinsman went rabid. It was not till then that the selectmen of the town took any action. They did not report the

case, but they tried to get the owners of dogs in the district to muzzle the dogs; and there the trouble began. The minister of the place was among the owners, and he absolutely refused to properly muzzle his dog. He said that his dog was muzzled if he had a muzzle tied round his neck, and that was all he was going to muzzle his dog, anyway. He said that Ben Butler had won a case in court on that very point. At that time the order read that a dog was to "wear a muzzle." Ben Butler won his case on the wording of the order, that the dog was to "wear a muzzle," the court holding that a dog was "wearing a muzzle" when it was hanging round his neck. Since then, of course, the wording has been changed. But the minister absolutely refused to properly muzzle his dog.

Last fall several cases of rabies occurred in the neighborhood of Lynn, Swampscott, and Salem. The chairman of the board and myself went down there; and we noticed — it was after a muzzling order had been issued — that a large portion of the dogs in the street were wearing the muzzles round the neck. They were not properly muzzled. The difficulty in carrying that order out is in consequence of the opposition of different people to muzzling their dogs. They seem to think it is a cruelty. It may be so in isolated cases; but, if the dog is properly cared for, and using proper precautions when the muzzle is taken off at home, I think that there need be no trouble at all about the muzzling of dogs. The muzzle is specially intended to be worn when the dog is on the street or at large.

This case at Ipswich that I referred to is a characteristic one, and rather an interesting one. This second dog, that went rabid on the twenty-first day of December, was found by his owner, Kinsman, in the barn in the morning, snarling and biting at his cows. He drove the dog off with a club. The dog went out of the barn, and bit two or three of the neighbors' dogs; and, finally, that day he was shot. There was no report made about it. The proper authorities did not know about it at all until, on the twelfth day of January, one of the cows went rabid. The cow was found in a paroxysm in the morning, was taken out from among the other cows, moved to a loose box, and there she burst the hinges off one of the doors, she broke three or four boards off the box-stall, she bit at the wood so that it showed the marks of her teeth, and she threw herself on

the ground and beat her head on the floor so as to knock the horns off and reduce the core of the horn to pulp. Finally, she reared up on her hind legs, and fell down and died. The cow was then hauled out and laid in the snow over night; and that probably helped to preserve the brain, which was taken out the following day and sent to Boston for inoculation. No results have yet developed from that case.

In considering the subject of treatment, the board came to the conclusion that, in consequence of the exorbitant charges at the Pasteur Institute, it would be a good thing if the State, either the State Board of Health or some State institution, should treat parties that had been bitten by rabid dogs in Boston. I think it would be a very good thing: it would not necessitate the cost of conducting a separate institution; and it seems to me that something like that ought to be done. There have been, I think, thirty-three cases of rabies throughout the State during the past year, in dogs and other animals. There was one case in which a horse developed rabies. A year ago there were several cases in cows which had been bitten by dogs. Although it is not increasing, yet it is certainly not being reduced. One of the principal difficulties is, as I say, the difficulty in getting people to muzzle their dogs and in properly enforcing the licensing of dogs. I think this is a very important thing. If the local authorities would enforce, strictly enforce, the licensing of dogs, it would greatly help to reduce the danger from the curs and stray dogs that run about almost every village, it would tend to reduce the danger from that class of animals.

Dr. H. G. Stetson.— I should like to ask Dr. Burr if he knows of any place in the country, besides the Pasteur Institute in New York, and one place in Baltimore, where treatment can be carried out for the prevention of an attack of rabies. I know that the charges at the Pasteur Institute are very large; and, as Dr. Burr says, to a man earning only $2 a day, it seems an enormous expense to assume merely upon supposition. I have often wondered if there was any place in the country, aside from those two places, where patients could be sent at a less expense. I don't know exactly what the charges are at the Pasteur Institute. Those

at Baltimore, I believe, are $150; and in addition to that there is, of course, the expense of keeping the patient for three or four weeks,— that is, his board and general living expenses while he is undergoing the treatment. Some four or five years ago I was in Baltimore at the time that Dr. Kyrle and Dr. William H. Welsh of Johns Hopkins were conducting their experiments in rabies and at the time that they were preparing to inaugurate their treatment for rabies; and I saw very many cases of rabies from inoculation experiments in rabbits. Those cases were pretty indelibly fixed upon my mind, so that it will be a long time before I forget them. Dr. Burr says one thing which I should most heartily concur in; and that is, by all means make people understand that after a dog has bitten a person they never should kill the dog. The dog should be allowed to live, and live under the closest observation. In that, in a measure, rests the treatment of the bite upon the person.

Dr. Cook.— I should like to ask the doctor, In how short a time after a dog has been bitten by a dog is it possible to find the virus in the saliva or in the mouth, and is it possible to find the virus there before he produces symptoms of rabies?

Dr. Burr.— I think the period of incubation varies considerably in animals. It may come on in ten days. Twenty-five days is perhaps to be looked upon as an average time. I think that certain cases have shown that the virus may be present in the saliva some time previous to the animal presenting symptoms,— how long I really can't say. In regard to the question of the previous gentleman, as to the places for treatment, I don't know of any place outside the places which he has spoken of.

A Member.— I should like to ask the Doctor if he has noticed any particular characteristic, any particular kind, class, or condition of dogs that are predisposed to rabies, or whether there have been any and all kinds that have come under his care and consideration.

Dr. Burr.— It is common to all dogs. Breed or class has nothing to do with this disease.

A Member.— I should like to ask Dr. Burr whether he thinks the treatment at the Pasteur Institute can be recommended as absolutely harmless, whether any cases of rabies result from treatment, whether any suspected cases may go there as the result of

treatment, and also whether he goes outside of the city of Boston to investigate suspected cases.

DR. BURR.— In regard to the last question, I can answer that very readily. We have no authority outside of the city of Boston. In regard to the first question the harm, if any, must be slight. I have never heard of any cases of rabies resulting from Pasteur treatment.

DR. SHEA.— I should like to ask Dr. Burr if the treatment at the Pasteur Institute is a specific one. Is it really in the same line with the serum treatment of diphtheria? Have there been any cases sent to the Pasteur Institute that have died with hydrophobia?

DR. BURR.— If there are any cases which died, which have been sent there?

DR. SHEA.— Yes, after having the treatment.

DR. BURR.— I don't know what the percentage of death is of cases treated at the Pasteur Institute, I am sure. I know that all cases are not successful, by any means; but it should be said that the treatment is not expected to be successful, if begun after symptoms of rabies have set in. In regard to the treatment, it is not on the same lines at all as the serum anti-toxine of diphtheria. The treatment consists in the injection of very dilute attenuated virus, and gradually working up to a virus equal to the normal strength of virus from rabid dogs. To go into it in detail, the rabbits are injected from rabid animals, and probably die within thirteen or fifteen days. Rabbits inoculated from this rabbit die in a shorter time. This is repeated from one to another until death occurs in about six days. That is probably the height of the virulence in rabbits. The cord of a rabbit dying within six days is removed, and divided into parts and dried at different periods, I believe, from one to thirteen days. While drying, it seems to lose some of its virulence; that is, dried for one day, there is a certain loss of virulence; two days, a certain loss, and so on. Cords dried thirteen days are comparatively weak. Injections are made subcutaneously into patients of an emulsion of cords dried for different periods, working up from the thirteenth day to the first day or cord dried for one day only. The treatment takes about two weeks. In that way the patient, as

I understand it, is able to stand the injection of a virus of about the same strength as the virus of the dog from which the patient had been bitten.

THE CHAIRMAN.— It strikes me, gentlemen, that Dr. Gage has asked a most practical question. Whether everybody feels satisfied as to what rules should be our guide, I am not able to say; but it strikes me as a matter in which every physician must be greatly interested. A child has been bitten, and brought to the doctor for advice. The question is asked, What shall be done with this child, immediately and afterward? Should this child be sent to the Pasteur Institute or some other place where treatment may be given, or not?

It is a very important question. And there is another question, which may have priority; and that is, What shall be done with the suspected dog which may be found dead? It strikes me that we can come to but one conclusion. If the dog is alive, he should be confined and watched until there is some satisfactory result as a guide. If the dog has been killed, find him; and, unless he should be found in a very advanced state of decomposition, his brain should be secured for examination. In the absence of finding the dog, there is a duty for some responsible person. Let the physician determine as to the probability of the dog being rabid; and, if the probability is strong, he should advise the parent to send the child for treatment. How can we do less, and do our duty to the parent, friend, or the patient himself?

Another question which struck me as very pertinent, indeed, in this line was concerning the trustworthiness and expenses of the different institutions where treatment is given. Perhaps Dr. Burr would be willing to ascertain at least the expenses of these different institutions, how many there are and where they are, and report to the Association as much information concerning them as may be practicable and pleasurable to him. He promises it.

Are there any other remarks to be made?

DR. GAGE.— May I ask just one question?

DR. DORMAN.— I should like to ask if there is any harm to come from the treatment.

DR. BURR.— I think I should rather refer that question to Dr.

Parker, who, I know, has been there for treatment. Perhaps he can give you some information on that point. I don't know of any harm that can come. I know Dr. Parker has told his experiences, and perhaps he would relate those again to you.

The Chairman. The question is, Is there any one present who can state that any harmful effects have been known to arise from the treatment, such as the Pasteur method of treatment?

Dr. Parker.— Mr. Chairman and gentlemen, so far as I know, there is no harm from the treatment; but there is a good deal of discomfort. I was down there, I think, about six years ago, and underwent the treatment for a bite from a rabid dog. The treatment was somewhat like the following. You went down there to the institute and there injections of the solution were made into each side. That was repeated in the evening again, and every following day for some days; and then it was done just once a day. You were not detained at the institution. You were allowed to take the rest of the time to yourself; but you were expected to go there at first twice a day, and then later once a day. The discomfort arose from the pain from the injections into the soft muscles. After being continued for several days, it got very painful. I can easily imagine how a child especially would seriously object to it. But, so far as any bad effects were concerned, I don't think there were any. When I first went down, I had to have the horse-car absolutely stopped when I got off at the institute, because every time I stepped off the car it would jar me from the top of my head down to the soles of my feet. That got worse for two or three days at first, and then gradually disappeared. But the discomfort principally arose from the soreness at the various points of injection. I believe there is no case on record where any person has come down with hydrophobia after the course of treatment there, unless he went too late. There are several cases where people have died from it, when treatment began after they had shown some symptoms of nervous disturbance; but I understand — and I think that Dr. Winchester is of the same opinion, he has been through the treatment, too — that their records show that no cases of hydrophobia have developed after the treatment, if begun in time.

Dr. Gage.— There is one question I should like to ask Dr. Burr. It is the question that the doctor is asked by the family, and I should

like to ask him to answer it for the Association. They say, Doctor, supposing this child of mine has been bitten by a rabid dog, how soon must he go down there in order to be safely treated; that is, what is the longest interval during which treatment can be safely delayed? That is one question we have to answer.

Dr. Burr.— I am under the impression that the Pasteur Institute is in the habit of telling patients that, if they come later than about ten days, they do not guarantee a cure. In the case of the boy that was bitten: he was bitten on a Saturday, and went to New York on the second following Sunday night, which was about eight days. I suppose he arrived at the institute on Monday. Those in charge at the institute told the father that, if he had allowed it to run two days longer, they would not guarantee a cure.

Dr. Parker.— Mr. President, if I might again take up the time for a second with reference to the Pasteur Institute in New York, my reason for advocating some method of treatment in this State would be simply the cost of the treatment. The principle of treatment is right, but the cost is excessive, especially for a poor man; and, if the State is going to undertake the preparation of anti-tetanus serum and serum for the treatment of diphtheria, it would seem as if something might be done in this other direction as well.

Dr. Smith.— I suppose that the members of this Association do not realize to what extent the anti-rabic treatment is being applied throughout the world. I think the statistics probably go into the hundred thousand cases treated up to date. There were some twenty-four anti-rabic institutes in the world in 1894. Almost every State in Europe has one; and, as you know, there are several in this country. So far as the statistics go,— and I have looked into the subject recently quite carefully,— there appears to be no danger from the treatment; and it seems to me that the advice that a physician should give, if there is any doubt whatever, is for the patient to undergo the treatment. That will relieve the physician of all responsibility later on.

As regards the expense, it must be borne in mind that every institution is under the same expenses, whether it treats two patients or one hundred patients, with the slight increase of *personnel* to make the injections and do the clerical work. It is absolutely nec-

essary to keep up the series of inoculated rabbits. It is necessary to take out the spinal cords of these rabbits at such and such a time, and prepare them for the injections. If they are dried longer than, say, thirteen or fourteen days, they are of no use, so that it is necessary to keep up the main work of the institute if there is not a single patient there. The rabbits must be inoculated at proper intervals; and, if there are no patients, the material would simply have to be thrown away. This should be borne in mind in criticising the excessive expenditures of these institutions, because there must be a plant with so many rooms for the preparation of this material, whether there are thirty cases in the year or a few thousand.

In regard to the appearance of the disease after treatment, it should be stated that the European statistics show that now and then the original disease breaks out during treatment and sometimes fifteen days after. It is generally assumed now that it takes at least twenty days after the beginning of the treatment for full immunity to be produced; and, if the disease of the patient is going to appear clinically during those twenty days, of course the treatment has no effect, so that the disease now and then, after severe bites on head and face, may be expected to occur a few days after the treatment is over. But, so far as the statistics go,— and I believe that most of the anti-rabic institutes are managed by men who furnished entirely reliable figures,— I think there is no evidence that there is any danger from the treatment; and, if I should be bitten by a susceptible animal, I think I should at once prepare to submit myself to the treatment if there was the slightest suspicion that the animal was rabid.

Dr. Burr.— Mr. Chairman, in regard to further answering Dr. Gage's question as to the length of time that might elapse before treatment was begun after a bite,— referring to the Melrose case, a certain veterinarian of Malden was called to see a dog in Melrose November 23, a woman having been bitten in church by the dog on November 14. At that time, on close questioning, by the veterinarian, of the owner of the dog, he could obtain no history of bite; and the owner insisted that the dog had a fish-bone in its throat, as they had had fish the day before at the house. He ventured to put his

hand into the mouth to remove the supposed fish-bone, and either scratched his hand or was bitten by the dog, I couldn't say which, but thought nothing of it at the time. The dog died the following day. Nothing was thought of it then. He went off, I believe, on an outing in Maine, and while there one night had a dream that he was being bitten on the finger by a dog. I believe he had at that time a spasm: whether it was a nervous spasm or not I don't know. He returned, and consulted friends, with the result that nothing was thought of it. About January 2 report came that the woman who had been bitten on the 14th of November by a dog had died of hydrophobia. In the mean time it was learned that the dog which had bitten this woman, also bit the dog seen by the veterinarian. The veterinarian immediately went on to the Pasteur Institute January 2, which is a lapse of some fourteen odd days. I believe he has had a number of spasms there: whether they are entirely nervous or not I cannot say. He was to return a week ago last Tuesday. He did not return on that day, but I have not heard that there are any serious results. He expected to be cured, I believe. That was forty days after the bite.

The Chairman.— Any further remarks to be made upon this paper? If not, Mr. Morse desires to say a few words concerning an incinerating plant recently erected in Boston, and which was put in operation yesterday.

## THE REFUSE DISPOSAL PLANT OF BOSTON.

Colonel W. F. Morse.— Mr. Chairman and gentlemen, as a good many of you know, I am an advocate for the disposal of garbage by cremation. Five years ago we discussed the merits of reduction and cremation at one of our meetings, and came to no conclusion. Two years ago we discussed it all the afternoon at Fall River. The arguments were fairly presented on both sides; and still it was not possible to determine which method was better, except that cremation was making progress, while reduction was going backward. At the present time, seventy-five or eighty or nearly one hundred cities are using cremating furnaces for garbage and waste. Five or six at the utmost are using the reduction process, and their numbers are being reduced. The result is that cremation is progressing, and reduction is receding.

What I had to say was this. The efforts of the builders of cremating furnaces have been to utilize the heat in a way to make it more effective. The late Colonel George E. Waring brought from Europe two years ago the idea of a long travelling belt, on which could be placed all the waste, from which parts could be picked out and sold, and the remainder burned. He tried the experiment in New York. He caused to be built a furnace. He collected from two hundred thousand people all the combustible and paper waste, separate from garbage and ashes, and sorted it over. After picking it out, he found that 10 per cent. only remained to be destroyed. 90 per cent. was salable, and valuable for marketable purposes. That experiment has been tried two years. It has developed two facts: one is that the combustible refuse of a given number of people is worth saving, provided it is large enough in bulk to be handled on a large scale by machinery; the second fact is that the combustible refuse of any city will furnish heat enough to burn the organic waste of the same community.

Boston has been peculiarly fortunate in having a progressive mayor, a progressive board of health, and a progressive corps of engineers. They tackled this question last April, advertised for bids, and got five propositions, which they promptly rejected because

they were not practical. After that there came to them in an informal way a proposition which they did put into use, and which has resulted in building within six blocks of this place a Refuse Disposal Plant, in a building which is 165 feet long, 75 feet wide, and to which is brought all the combustible refuse of every character and kind from about two hundred thousand people. This building contains a long travelling endless belt of steel plates. The carts dump upon the first twenty-five feet of the belt. It travels along 100 feet further; and during the time it is passing twenty-five men on each side pick out five or six different classes or kinds of paper scraps, rags, leather, glass, iron, everything that is marketable and salable. After they have picked this over, they have removed 80 to 90 per cent. from the whole quantity. The remaining 10 per cent. goes by an inclined conveyor into a bin, and from there is put into a large furnace, which furnishes heat to move the conveyor and to light and heat the building. This plant was built at a cost of about $30,000. It was yesterday for the first time put in operation. They brought about 150 cubic yards of waste, and at noon the whole quantity was sorted and burned; and the men were waiting for more. To-day, I am informed, they will bring about 250 yards,— a pretty fair test of a plant for the second day of beginning work.

We do not know how much heat this will generate. We do know this: that, when you take a piece of paper and burn it, you get a degree of heat equivalent to a certain amount or proportion of coal which is the standard of measurement. It does not go as far as coal; but it is certain that there is enough heat which, if used in a proper way, can be utilized for the destruction of matter other than itself. I believe it to be the case that every garbage furnace in the country is throwing up its chimney at the present time waste heat which, if properly utilized, could burn the garbage itself over again. I believe also that this point has been attained: that every city or town of a population of ten thousand, with a proportional amount of garbage,— we will say, for instance, five tons per day for a population of that number, and an equivalent amount of combustible waste,— if it will bring to one point the garbage and the refuse separate, one can be made to destroy the other without cost. If a city of fifty thousand people, or any community of whatever size, will perform the same

thing in the same way, making three separations,— the garbage, the refuse, and the ashes,— it can use the worthless refuse to burn the garbage, and can sort out from the refuse itself more than enough marketable product to pay the expenses of the whole. That means that the work can be made self-supporting, and that is the object and the point that we are all after. This will presently be determined. At this time the question is in such a shape that in the course of the observation of this plant here, coupled with one or two others that are about to be built, we shall be able to state positively that with a given quantity of combustible refuse, weighing a certain amount and occupying a certain space in bulk, we can destroy another given quantity of organic refuse within a given time and at a given cost. That is the point we are after. Five years ago we had begun, two years ago we were progressing, and in a year from now I believe that almost every one of the cities in New England will utilize its combustible waste to destroy its organic matter at a cost which is practically doing it for nothing.

The Chairman.— I want to say before adjournment, gentlemen, that there is still a trifling lack of interest in having the Association meet at different parts of the State. I don't blame you for wanting to come to Boston: that is the place to come; but you don't want to come to Boston every time. Boston does not want to appear to be selfish in this matter either. If any city in the Commonwealth wants the Association to meet there, you have only to say so; and we want you to feel at perfect liberty to give the invitation, and say when you want us to come. I won't name the cities, but there are many of them that I am afraid hesitate to give the invitation.

I don't want to say it for publication, but it has been on my mind ever since the Treasurer read his report to-day that we should congratulate ourselves on the remarkable progress we have made in the last nine years. Starting so modestly and simply as we did, with nothing in the treasury and with a small number of earnest workers in this organization, to-day we are much more than double in numbers and with something like $700 in the treasury, and with an earnestness for work such as has never at any previous time existed. Still, we never can afford to slacken our energies at all in building

up and supporting an organization like this for good work. Let us look still for larger membership, but particularly a membership of working men; and let each one suggest living topics, topics which interest the boards of health. Let every one feel an interest and a freedom to suggest questions which he wants to hear discussed at these meetings, and there can be no question as to the life and vitality of the Association doing that kind of work.

A motion to adjourn would be in order.

(Adjourned.)

# JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH

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## April Meeting, 1899

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SUBJECTS: On the Presence of the Typhoid Bacillus in the Urine, and its Relation to the Public Health — Some Questions Relating to the Bacteriological Diagnosis of Diphtheria.

# Members Massachusetts Association of Boards of Health.

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## THE JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

The Massachusetts Association of Boards of Health was organized in Boston in March, 1890, with the following objects: the advancement of sanitary science in the Commonwealth of Massachusetts; the promotion of better organization and co-operation in the local Boards of Health; the uniform enforcement of sanitary laws and regulations; and the establishment of pleasant social relations among the members of the Association.

All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

The Official Journal of the Association is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them. No part of this matter is printed in any other periodical.

The Journal will present, from quarter to quarter, a fair and adequate picture of the progress of practical sanitary science as applied to the needs of a modern community. The various subjects which are reviewed in the quarterly meetings of the Association are treated by experts qualified to speak from daily experience in Public Health offices, who, as men of science, are careful to be scientific and comprehensive, and who, as public officers, are no less careful to speak pertinently and so as to be easily intelligible to the layman.

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## APRIL MEETING

OF THE

## Massachusetts Association of Boards of Health.

The April quarterly meeting of the Massachusetts Association of Boards of Health was held in Boston at the Parker House on the afternoon of Thursday, April 27, the President of the Association in the chair. Following is a report of proceedings : —

The President.— The Secretary of the Association has been unexpectedly called away by death in his family, and I will ask you to nominate some one to temporarily take his place.

Dr. Chase.— Mr. President, I would nominate Dr. Frank P. Denny, of Brookline, for temporary Secretary.

The motion was seconded by Dr. Durgin, and adopted.

The President.— The Executive Committee reports to the Association the names of the following candidates for election to membership : Dr. A. A. Etienne, a member of the Board of Health of Ware, Mass.; Dr. E. P. Pierce, of Springfield, Mass.; Dr. Atherton P. Mason, of Fitchburg; Dr. Manual V. Sylvia, of the Board of Health of New Bedford; and Dr. Thomas B. Smith, of Lowell. Is it your pleasure that these candidates be elected to membership in the

Association? If so, you will signify it by saying, Aye: contrary-minded, No; and they are so elected.

The reading of the records will necessarily be dispensed with, as the records are not here.

The miscellaneous business of the Association is now in order. I should like to ask if there are reports of any committees. Dr. Burr, I believe, has a report to make on behalf of a committee.

## ANTI-RABIC INSTITUTIONS.

Dr. Burr.— Mr. Chairman, I believe I was appointed a committee of one to inquire and report at this meeting the number of anti-rabic institutions in this country and the fees for treatment. I sent out questions to the State Board of Health of each State in the Union, and also a number of the boards of health of Canada and Mexico. The questions were as follows: "Are there any anti-rabic institutions in the State?" "How many are private?" "How many are public?" and "The fees for treatment?" I received reports from all the States with the exception of some eight or nine. No reports were received from Alabama, Arkansas, California, Indiana, Mississippi, Nebraska, North Dakota, South Dakota, Texas, or West Virginia. Of all the reports received, only three States report anti-rabic institutions: one at the Pasteur Institute of New York, one in Baltimore, and one at Chicago. All three are private institutions. There are only three of them, so I will read the letter of each one.

"The only Pasteur Institute in Illinois is the one directed and founded by me." This is from Dr. Lagorio. "It is a private institution, and there are no State institutions of the kind. The fees for the complete Pasteur treatment are $100. Of course, many indigent poor have received the treatment gratis."

The Pasteur Institute of New York is that conducted by Dr. Gibier. He writes as follows: —

"In answer to your letter of the 28th of March, we beg to say that our regular fee for preventive treatment of hydrophobia is $200, including board and room for fifteen days. This is the amount paid to us by the State of New York for its indigent patients."

And there is one from Baltimore, as follows: —

"We have a very good Pasteur Institute here at the City Hospital, Saratoga and Calvert Streets, under the direction of Dr. N. G. Kierle." He does not give the fees.

The result is that there are three anti-rabic institutions in the country, all three being private institutions. The fees are from $100 to $200 for treatment.

The President.— I should like to ask if it is not true, Doctor, that Dr. Biggs has lately made a provision in New York, on behalf of the city of New York, for the treatment of these cases.

Dr. Burr.— I wrote to the New York Board of Health direct, because I had in mind that there was some sort of an institution connected with the Health Department proper; and I received the following letter: —

"Yours of the 28th inst. requesting information respecting institutions in the State of New York for the treatment of rabies is received; and in reply I beg to state that your communication has been forwarded to Dr. Paul Gibier, No. 313 West Twenty-third Street, who will doubtless be able to give you the information desired."

The President.— I asked the question because Dr. Theobald Smith told me the other day that in a recent conversation with Dr. Biggs at the institute he was informed that they did intend to undertake the treatment of people suffering from supposed rabies or liable to the infection. If they do, it would be a great assistance, because it would probably be the most thoroughly conducted treatment of the sort in this country.

Dr. Burr.— In one letter which was received from Baltimore it was stated that in New York they had been using the virus made by Dr. Kierle of Baltimore; that is, the Municipal Laboratory in New York were using the virus made by Dr. Kierle of Baltimore, although in the letter received from New York Board of Health nothing is said in regard to it.

The President.— Has any other committee anything to report? Dr. Durgin?

## THE INSPECTION OF MILK.

Dr. Durgin.— As chairman of the Committee on Legislation, I have very little to report. I would say that the last time the committee made any appearance at the legislature was about a year ago, and then in behalf of what was proposed in the Association, that the appointment of milk inspectors should be transferred to the boards of health. We were badly whipped. There seemed to be some great fear in the committee as to what all this meant; and I can assure you that, while the proposition made there seemed to us extremely simple and honest, the appearance of the committee did not indicate to me that they felt so. Those whom we had taken into our counsels and thought they were with us were there in force against us, and with them leaders from some of the inland cities and towns. I think it was a mayor who put in a very ponderous appearance in a political and very boisterous way, and accused the boards of health of being what they should not be. He made much of the fact that in his city, I think it was Lowell —

A Member.— Guilty.

Dr. Durgin.— He had vigorously exercised his prerogatives over his board of health, and — At any rate, we were glad to retire whole. The Boston Board of Health, after this, having sufficient legislation for its own use, put in force those regulations which you all approved. We trimmed them a trifle, and adopted them last December. We have put these regulations in force in the interest of producers and consumers, and hope to see the farmer better paid and the consumer served with cleaner and safer milk. I have had some of the regulations and blanks brought over, and perhaps you would like to take them home with you. We have the assurances now of the milk contractors, whose interests are identical with ours and those of the consumers, that they will co-operate with us; and I have no doubt that we shall get on harmoniously. I hope that the other boards of health will soon find it possible to follow suit, for there cannot be a much more profitable line of work than in the care of the milk supply.

The President.— Is there anything else under the heading of Miscellaneous Business to come before the Association at this time?

Dr. Bigelow.— Mr. President, I don't know but it is a little too soon to begin to speak about another feed, when we are all theoretically full.

The President.— I think the Association will pardon the allusion.

Dr. Bigelow.— But there is a little town in Massachusetts, a few miles out of Worcester and not far from Boston, which goes by the name of Leominster. The Board of Health of Leominster would like to put in an application for the State Board and the local boards to meet with us in October for the quarterly meeting and for a feed. We have no sail down the harbor, we have no Parker House; but we will try to give you a good feed and show you what attractions there are in that vicinity. I hope that this will be received by the Executive Committee and the members willingly, and that the Association will vote to have the October meeting in Leominster with the local board of health of that town.

The President.— I understand that there has been another invitation informally extended to the Association for this meeting; and I presume the usual course will be followed if there be no objection, and that the Executive Committee will report to the next meeting of this Association their own feelings on that subject. If there is no objection, it will be understood that the invitation is referred to the Executive Committee for the purpose of report to this Association at its July meeting. Is there anything else under this heading, Miscellaneous Business?

## ANTI-RABIC INSTITUTION FOR MASSACHUSETTS.

Dr. Gage.— Might I interrupt a moment to ask Dr. Walcott if the State Board of Health of Massachusetts have taken any action toward establishing an anti-rabic institute in this State, or whether it is their intention and purpose,— if I am not getting ahead of time?

The President.— No, the State Board of Health in that matter holds itself entirely at the disposal of the local authorities of Massachusetts; and, if there appeared at any time to be a sufficient justification for the expenditure of money in that direction, we should be very glad, indeed, to go before the legislature and ask for the appropriation. It was somewhat with reference to this matter that Dr. Smith, the pathologist of the State Board of Health, has been making a recent examination of some of the more prominent institutions in the way of public work in the field of preventive medicine.

Dr. Gage.— May I interrupt again? If it is not out of order, I should like to make a motion in reference to this question. I should like to move that it is the sentiment of this Association that it is necessary and expedient that such an institute be established in this State by the State Board of Health.

Dr. Durgin.— I second the motion.

The President.— You have heard the motion of Dr. Gage. Is there anything to be said by any member of this Association upon that subject? It is moved and seconded that it is expedient for the State, through its State Board of Health, to undertake the care of persons infected by rabies or supposed to be infected by rabies. If that be your pleasure, you will signify it by saying, Aye; contrary-minded, No. It is so voted.

If there is no other business, we will then proceed to the regular programme for this afternoon's exercises. The first paper is a paper on "The Presence of the Typhoid Bacillus in the Urine, and its Relation to the Public Health," by Mark W. Richardson, M.D. I have the pleasure of introducing to you Dr. Richardson.

# ON THE PRESENCE OF THE TYPHOID BACILLUS IN THE URINE, AND ITS RELATION TO THE PUBLIC HEALTH.

BY MARK WYMAN RICHARDSON, M.D., BOSTON, MASS.

It is the purpose of this paper to present to this Association briefly the results of bacteriological investigations which the writer has made upon the urines of persons sick with typhoid fever. These investigations have been made in the wards and in the pathological laboratory of the Massachusetts General Hospital, and have required for their completion about a year and a half. Accounts of the work have been published in the *Journal of Experimental Medicine*, 1898, No. 3, and 1899, No. 1. Any one desiring details of the investigations may seek them in this journal.

To take up the most important literature briefly: —

Neumann (1890) examined the urines in forty-eight cases of typhoid fever, and found bacilli in eleven instances. The bacilli were always present in large numbers, and in some cases persisted far into convalescence.

Wright (1895) obtained bacilli in six out of seven cases.

Besson (1897) found typhoid organisms in six out of thirty-three cases.

Horton-Smith (1897) examined the urines of seven cases, with three positive results. Horton-Smith also pointed out the fact that the bacilli can be demonstrated, oftentimes, in cover-glass preparations from the urine, and, further, that the organisms may be so numerous as to render the urine distinctly turbid.

Petruschky (1898) obtained three positive results in fifty cases. The bacilli remained in one case two months after the beginning of convalescence; in a second case, six weeks; and, in a third case, eight days after the temperature had become normal. Petruschky estimated that in one case a cubic centimeter of urine contained 170,000,000 bacilli.

Cushing has seen a case at the Johns Hopkins Hospital in which five years after typhoid fever the urine showed evidence of marked cystitis and typhoid bacilli in pure culture.

The cases investigated by the writer number one hundred and four, with twenty-three positive results. The conclusions reached agree entirely with those of the investigators just enumerated.

1. Typhoid organisms are present in the urine of about 25 per cent. of the cases.

2. The number of the bacilli is often enormous. Indeed, the urine may be distinctly cloudy simply from the organisms present.

3. The bacilli may persist in the urine for weeks, months, or even years after convalescence begins.

4. The virulence of the bacilli obtained from the urine was tested in one instance, and their pathogenic power equalled in every respect that of cultures obtained from the internal organs and the stools.

5. The urines of typhoid patients should, therefore, not only be rigorously disinfected during the disease, but should also be supervised carefully during convalescence.

Studies upon methods for remedying this condition resulted as follows: —

1. Irrigation of the bladder with boric acid (5 per cent.) was of no avail. Corrosive sublimate (1.7000) was effective.

2. Salol by the mouth, ten grains t, i, d, was effective in a single case. In two cases the bacilli persisted in undiminished numbers.

3. Urotropin was used in nine cases in a dosage of ten grains three times daily, and with excellent, not to say extraordinary, results. In every instance the organisms were removed. Moreover, this happy result was obtained oftentimes in twenty-four to thirty-six hours; and a urine which had been cloudy with bacteria would in two days become absolutely clear. In two cases, when the urotropin was stopped, the bacteria returned to the urine; but a second or third dose of the drug sufficed to remove them entirely. In this connection I will read a short note which I received recently from Horton-Smith, who, as already stated, has worked upon this same subject at St. Bartholomew's Hospital in London, and to whom I had written of the virtues of urotropin. He writes: —

*Dear Doctor*,— I am just sending this to say that we have just tried urotropin in a suitable case. The typhoid bacilli were present in enormous quantities, and

ten grains of urotropin were given three times a day for two days. At the end of that time the urine was sterile. Very many thanks for letting me know of the drug.

We have, then, before us a condition, and a remedy for that condition,— a condition the importance of which was recognized nearly ten years ago, but which has never received the attention which it has deserved. In fact, it has been my experience that the urines in typhoid fever have been utterly neglected, while the stools were rigorously disinfected. In my opinion, however, the urines are much more to be feared than the stools. To be sure, only 25 per cent. of the cases show infected urines; but the enormous numbers of the bacteria, and their persistence into convalescence, make them extremely dangerous, not to the patients themselves,— for they are, of course, immune,— but to those in their environment.

We keep our diphtheria patients in quarantine until their throats are free from Klebs-Löffler bacilli. Is it not just as important that typhoid convalescents should be subjected to equally strict supervision? The answer to this question must be in the affirmative.

How shall this supervision be carried out?

In the first place the urines as well as the fæces must be carefully disinfected throughout the disease. Then, as the temperature reaches the normal point, the urine should be examined bacteriologically, to determine the presence or absence of bacilli. If no bacilli are found, then, of course, no active measures are indicated. Before the patient is allowed to go abroad, however, — that is to say, after from two to three weeks,— a second and final examination should be made.

If the urine at the beginning of convalescence shows typhoid bacilli, then urotropin should be given in ten grain doses three times daily for a week. A week after the administration of the urotropin has been stopped, the urine should be examined again. If there are, at that time, no bacilli present, the patient may be released from supervision.

Finally, it seems to me more than probable that during the coming summer and fall we shall see an unusual number of typhoid cases. The reason for this will lie in the fact that so many soldiers convalescent from typhoid have returned from Cuba and Porto Rico.

These soldiers will be responsible in part because of typhoid organisms persisting in the stools; but the danger of this method of infection will, in my opinion, be surpassed a thousand-fold by that of the 25 per cent. of typhoid urines which, entirely unsuspected, have been deposited broadcast through the camps and the country in general.

I have here some typhoid urine which very fortunately turned up at the hospital this morning, and which shows the cloudiness which these typhoid urines may often show, very largely from the presence of bacilli alone. In this there is a certain amount of sediment, which of course accounts for part of the cloudiness; but most of it, I am sure, is due to the bacilli alone. Also, I have a couple of plates here which will demonstrate very forcibly, I think, the numbers of these bacilli. This first plate represents the number of bacilli in a single platinum loop of urine,— that is to say, the ordinary bacteriological platinum loop, which is, of course, a very small amount; and yet you can see, by holding it in the proper light, that in the plate there must be thousands of colonies, each colony, of course, representing the growth from a single bacillus. The second plate represents approximately a cubic centimeter of urine, and you will see that the bacilli were so numerous that the individual colonies cannot be made out at all.

The President.— You have heard this most interesting communication of Dr. Richardson's. Is there any discussion upon the matter, or are there any questions to be asked, which, I presume, Dr. Richardson will very gladly answer?

Dr. Brown.— Mr. Chairman, I am quite unfamiliar with the drug mentioned by the doctor. I should like to know if it has any other effect than the destruction of the bacilli in the bladder, if there are any other therapeutical indications, or if it accomplishes any other result, and if there are any contra-indications to its use.

Dr. Richardson.— Urotropin is a comparatively new drug, and was first got out by Nicolaier in Göttingen. Nicolaier first claimed that it was an especially good drug for the solution of uric acid calculi, but this claim has not been verified by other observers. But, although it has not been able to dissolve calculi, it has been found to be a very strong urinary antiseptic, and has been used very largely in

cases of cystitis, especially of those due to the colon bacillus and its varieties. On cystitis due to tuberculosis or to gonorrhœa it is said not to have much effect. Urotropin is said to be a combination of formaldehyde and ammonia, and it is excreted in the urine partly as urotropin and partly as active formalin.

Dr. Bigelow.— Mr. President, I should like to ask a question. I should like to ask if this drug, urotropin, is supposed to modify or to cut short the progress of the disease by destroying the bacillus in the urine. If given in the earlier stage of the disease, will it cut short the progress of the typhoid fever?

Dr. Richardson.— I don't think that it has ever been tried in that way. There is certainly no literature on the subject.

Dr. Miller.— Mr. President, I think this is one of the most interesting papers we have had in a long time. It takes up this subject in a new light. While for years those of us that have treated typhoid fever have been very particular that the stools be disinfected and disposed of, the mass of us have never thought to be particular about the urine. As the doctor has just suggested, typhoid fever patients that have the urine loaded with this bacillus may deposit the urine around almost anywhere; and probably that is one great cause of the spreading of typhoid fever. I, for one, am very glad that the doctor has taken up this subject. It opens it up in a new light. I hope that in some hospitals, where they have a chance to test it, this drug, urotropin, will be used in the early stages of typhoid fever, with a view of seeing if it will not shorten the disease. To me this is a very interesting subject, because we have in many parts of the State considerable typhoid fever.

Dr. Gage.— Mr. Chairman, I should like to ask Dr. Richardson what the urotropin is given for. Do the bacilli present in the urine always produce symptoms referable to the bladder of the nature of cystitis?

Dr. Richardson.— No. It is quite a curious fact that in these cases in which the urine may be loaded with bacilli it is comparatively rare that there is an actual cystitis.

Dr. Gage.— The practical question which comes home to us who have a case of typhoid fever is this: Are we going to give urotropin in every case of typhoid fever or are we not, and how are we going to

be able to give it? It is going to be difficult for us at home to know whether there are any bacilli present in the urine. I wonder what clinical evidence would be of use to the physician to determine the need of urotropin.

Dr. Richardson.— I think it is practically impossible to tell in every case whether or not a typhoid patient has bacilli in his urine without a bacteriological examination. Hence, unless such examination can be made, I think that all cases should be given the urotropin for a week; and, in that way, I feel sure that certainly ninety-nine cases out of a hundred would be relieved of their dangerous character.

Dr. Miller.— Mr. Chairman, I should like to ask the doctor one question, with your permission,— whether those cases of typhoid fever where this bacillus has been found abundantly are more severe than the cases where none are found.

Dr. Richardson.— Yes, I think that much the larger percentage of infected urines occur in the more severe cases; that the comparatively light cases are those which will show a smaller percentage of urine infected by bacillus. I should like to say also that I think the cases of epididymitis and orchitis — which occur, rarely, to be sure, in typhoid — are ascending infections from the urine. Certainly, the two cases of epididymitis which I have seen in the Massachusetts General Hospital in the last two years have been cases in which motile bacilli were present in the urine. In the first case, it was the typhoid bacillus: in the second case, it was a motile variety of the colon bacillus.

Dr. Brown.— Mr. Chairman, there is one more question I should like to ask, and that is whether this drug disinfects the intestinal canal.

Dr. Richardson.— That I cannot say. I don't think that has ever been thoroughly worked out.

Dr. Durgin.— It strikes me that the practical utilization of this information would be to require a bacteriological examination in a typhoid case before discharge as much as in the case of diphtheria. We used to discharge our cases of diphtheria when they appeared to the naked eye to be free from the disease, but we found it to be a mistake; and we now require before discharging from isolation that there shall be a bacteriological examination which will reveal the fact

that the diphtheria bacilli have gone. Now, if we are to utilize the facts given us to-day, we must require a bacteriological examination of the urine of the typhoid patient, to show that the presence of the typhoid bacilli have gone, before the patient can safely be discharged from treatment or excused from the disinfection of his urine.

Dr. Chapin (of Springfield).— I should like to ask, Mr. President, how long bacilli are present in the stool, whether or not it would be necessary to make daily or weekly examinations of the stools of the patients before discharging them, or of the milk, or of the saliva.

Dr. Durgin.— Well, we have to take up one thing at a time. We are hitting the urine now. If we should find that the bacilli are not gone from the fæces after the person appears to be well, we ought to apply the test there.

Dr. Richardson.— Mr. Chairman. I should like to say that what work has been done upon the fæces has tended to show that the bacilli disappear from the fæces very quickly after the temperature reaches the normal point. As far as our present knowledge goes, therefore, the stools of convalescent typhoids are little to be feared, when compared with the urines.

A Member.— I should like to inquire for the technique of the examination.

Dr. Richardson.— My method of procedure was as follows. In the case of male patients the glans penis was thoroughly scrubbed with corrosive sublimate (1.1000), and then the patient was told to pass his water in two portions; and only the second portion of urine was used for bacteriological examinations. Female patients were catheterized with all antiseptic precautions. The urine should, of course, be passed into a sterile vessel of some sort. Then two agar plates were made, one consisting of a single platinum loop of urine and another of approximately a cubic centimeter; and I found these two plates sufficient for my purpose.

Dr. Connell.— I should like to ask the doctor how soon in the course of the disease the bacteria appear in the urine.

Dr. Richardson.— They do not appear, as a rule, until the later stages of the disease, generally about the third week. I think that the large number of organisms in the urine is not due to the excre-

tion as such, of course, of so many bacilli. I think it is due rather to the excretion of a few bacilli by the kidneys, and then to their multiplication in the bladder.

Dr. Hill.— I should like to ask Dr. Richardson what confirmatory tests he used.

Dr. Richardson.— In testing the typhoid cultures which were isolated, the following methods were used: —

1. Bouillon for size, shape, and motility = motile.

2. Sugar agar for gas production = no gas.

3. Potato — visible or invisible growth = invisible growth.

4. Gelatine — presence or absence of liquefaction = no liquefaction.

5. Peptone solution — production of indol = no indol.

6. Litmus milk — acid production and coagulation = sl. acid production no coagulation.

7. Reaction to typhoid serum = positive in every case.

Dr. Chapin (of Providence).— I did not quite understand from Dr. Richardson whether there were any contra-indications at all to the use of urotropin, and whether it can be continued for quite a while without inducing any symptoms, or whether there are any other physiological actions of the drug.

Dr. Richardson.— As far as I have been able to learn from the literature and from my own experience, there are no bad effects from urotropin. A great many of these cases of typhoid showed a certain amount of albumen in the urine, and also casts; but I never could see that the amount of albumen or that the number of casts was increased. That has been the experience of other observers also. The dose as generally given is ten grains three times a day, but the drug has been given up to eighty or ninety grains a day. With large doses there has been some complaint of burning at the neck of the bladder, and perhaps the appearance of a few blood globules in the urine; but, with this exception, I have never seen any record of any bad effects from the drug.

Dr. Bryant.— I should like to ask if you have noticed any difference in this use in acid or alkaline urine.

Dr. Richardson.— The opinions are rather at variance on that point. Some say it works better in an acid urine, others say it

works better in an alkaline urine. Certainly, in most of those cases of typhoid, the urine has been acid. On the other hand, urotropin is said to be especially good in the cases of cystitis due to ammoniacal fermentation. It would seem, therefore, that the reaction was not especially important.

The President.— Is there anything else to be said upon this most interesting subject before we pass to the next paper upon the programme? I certainly desire, in the name of the Association, to thank Dr. Richardson for this most interesting contribution to our meeting [applause].

The next is a paper upon "Some Questions relating to the Bacteriological Diagnosis of Diphtheria," by Dr. Hill, whom I now introduce to you.

## SOME QUESTIONS RELATING TO THE BACTERIOLOGICAL DIAGNOSIS OF DIPHTHERIA.

### From the Bacteriological Laboratory of the Boston Board of Health.

BY HIBBERT WINSLOW HILL, M.D., DIRECTOR.

*Mr. President and Gentlemen,*— During a meeting of this Association at Gallop's Island, last summer, the desire was expressed by some of the members present for a fuller account of certain features of the diphtheria diagnostic work in Boston, such as could only be obtained from records reaching over a considerable period. That desire I have endeavored to meet in this article. There is, perhaps, little that is new or startling in the figures I have to submit; but they are of interest as indicating the actual possibilities and limitations of this work, when carried out for large communities and under modern conditions.

The data, other than the bacteriological results proper, from which these conclusions were drawn were supplied to us by physicians in the form of returns made on the cards which accompany the diphtheria outfits. It will not be surprising to you to learn that these

returns are often incomplete, and that some of them at least are inaccurate. Nevertheless, they furnish information which it would be difficult or impossible to obtain in any other way. Our cards read as follows: —

FORM 4. [*To be filled out by the Bacteriologist only.*]

**BOSTON HEALTH DEPARTMENT.**

**Diphtheria:** Case No. .............................. D .............. Received ..............................

..............................................................................................................................

..............................................................................................................................

---

[*To be filled out by Physician only: see other side also.*]

Date of Culture ....... Hour ........ A.M. ........ P.M. For Diagnosis? ....... For Release? .......

Patient's Name ............................................................ Age .............. Sex ......................

Patient's Address ..............................................................................................................

Date of Earliest Symptoms .......................... Source of Infection ............................

On the reverse the card reads as follows: —

Membrane present? ........... Nares? ....... Pharynx? ....... Tonsils? ....... Larynx? .............

If no membrane exists, state site of inflammation ................................................

Constitutional Symptoms ........................................................................................

Clinical Diagnosis ..................................................................................................

Remarks ...................................................................................................................

Physician's Name ....................................................................................................

Physician's Address .................................................................................................

[*Please fill out both sides of this Card as indicated, for every Culture made.*]

It will be seen that, carefully filled out, such a card supplies us with a great deal of useful information. Fortunately, a sufficient number are so filled out as to make the statistics compiled from them useful, although far from perfect. In illustration of some of our difficulties, I may be pardoned for referring to a few of the more common omissions and mistakes.

The date of earliest symptoms, for instance, is often left out. Sometimes the date, when given, is not the same on any two of a series of cards coming from the same physician and relating to the same patient. The source of infection is hard to fix in most cases, so that omissions there can be readily forgiven. One or two cases, however, in which the clinical diagnosis was entered as diphtheria gave also the source of infection as "a case of tonsillitis next door." It is hard to understand quite what this means. Perhaps the physician intended to imply that the diagnosis of tonsillitis made on the next-door patient was at fault.

The most serious source of trouble, so far as the executive work of the laboratory is concerned, preventing especially the prompt report to the physician which is so necessary and desirable, lies in the omission by the physician of his own name and address or of the patient's name and address. This is, of course, a clear case of oversight on the physician's part; and one can quite understand how, in the rush of practice, it may sometimes occur. But it is none the less apt to cause delay and trouble. One physician who requested urgently a prompt report on his culture omitted his own name and address, and also that of the patient, while giving otherwise a full account of the case. The report has now been waiting for him about six months, during which time he has perhaps treasured up against the laboratory some hard feelings, quite naturally, but of course, also, quite wrongfully. We have no clew to the identity of physician or patient under such circumstances, and cannot report.

By confusing the blank spaces left for the physician's name with that left for the patient, one big, heavily bearded man described himself on our records as a "female, aged seven years"! A good many cards come in without any designation as to whether the cultures they accompany were taken for diagnosis or for release. If the date of earliest symptoms be omitted also in such cases, a good deal of unnecessary trouble may be caused. I do not want to give the impression that the majority of physicians are careless about this matter of furnishing data. Many men are very careful and very conscientious about it. But there certainly are a few men in every city and town who do not stop to consider how important is all of the information asked for or how absolutely essential at

least some of it is. A carefully filled out card is one step in scientific advance,— a small one, certainly; but every little helps. If the bacteriologist is able to make every culture he receives tell a little in rendering diagnostic work better, more exact, and more intelligible, it reacts to the advantage of the public and of the profession, so that, in attempting to impress the importance of this feature of the diagnostic service upon the profession, I am but attempting to provide against one of those sources of error which retard progress, and retard it unnecessarily.

After this explanation the results we have obtained during the last nine months will be understood as representing an approximation to the actual facts,— an approximation only, it is true, but nevertheless an approximation of far more value than mere guess-work or the "general impressions" which one may form without the aid of fairly correct figures.

Turning now to our records, it is generally conceded that a positive culture for diagnosis is practically absolute evidence of the presence of diphtheria bacilli in the lesion from which the culture came, while a negative culture for diagnosis is not such good evidence of their absence. In a former paper the *a priori* reasons for this proposition were given. I shall now present the actual figures: —

TABLE I.

| | | |
|---|---|---|
| Total cases for diagnosis: bacteriological diagnosis, positive | 336 | 100% |
| Bacteriological diagnosis, positive on first culture | 312 | 92.5 |
| Bacteriological diagnosis: negative on first culture; positive on following cultures | 19 | 5.6 |
| Bacteriological diagnosis: unsatisfactory on first culture; positive on following cultures | 5 | 1.9 |

It will be seen that, out of all the cases for diagnosis which proved positive finally, the bacilli were found in 92.5 per cent. with one culture. Of these 336 total positive cases, the bacilli were probably present at the time of taking the first culture in most, and certainly were present later in all; but, nevertheless, about 5.6 yielded negative cultures from the first tubes taken. Those yielding "no growths" do not furnish any information to the physician; but the

negative cultures, about 5 per cent. of the whole, yield actually misleading information. This table then emphasizes once more the fact that a positive culture is reliable evidence of the presence of bacilli, a negative culture less reliable evidence of their absence. So far as these tables may be depended upon, about one case of diphtheria in twenty will yield a negative culture early in the attack. The natural conclusion is, then, that, if the physician has good clinical evidence that a case is really diphtheria, he should not readily reverse his diagnosis on the receipt of one negative culture. Two or three negative cultures should be obtained first.

It is also, I think, well known that a negative culture for release is less valuable than a negative culture for diagnosis. The reasons for this also have been already pointed out before this Association.* The following table, when compared with that already given, brings out this point clearly: —

TABLE II.

*A.*

| | |
|---|---|
| Cases released on two or more consecutive negatives: | |
| Total cases | 415 |
| Showing positive cultures at some time or other | 257 |
| Reported as diphtheria, without bacteriological diagnosis, but subsequently released by two negatives | 158 |

*B.*

| | | |
|---|---|---|
| Cases showing premature negatives: | | |
| Total positive cases released | 257 | 100% |
| Positive cases showing premature negatives | 74 | 28.6 |
| Positive cases not showing premature negatives | 183 | 71.4 |

In Table A the total cases released by us on two consecutive negative cultures are given. Some of these yielded positive cultures for diagnosis in the first place, then ran their course, and were released by negative cultures. Others, however, had no cultures for diagnosis, but were reported as diphtheria on purely clinical evidence, and did not pass through the bacteriological tests until they came up for release. Of these latter, some showed positive cultures during the release tests, others did not. In Table A those cases which showed

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positive cultures at any time during their course, and were therefore positively identified as cases in which the bacilli were present, have been separated from those which showed negative cultures only, since the latter cases cannot definitely be known as diphtheria at all. They or some of them may have been diphtheria, but there is no bacteriological evidence on the point. In Table B, therefore, I have taken only the cases known to have shown diphtheria bacilli. Of these 257 cases, 74 showed negative cultures, followed by positive cultures. In other words, 28.6 per cent. of the positive cases showed negative cultures for release during the time the diphtheria bacilli were really present. From this the point is established that a negative culture for release is not as reliable an evidence as a negative culture for diagnosis, since a negative culture for diagnosis occurs in only 5 to 10 per cent. of cases where diphtheria bacilli are present, while a negative culture for release occurs in a percentage three to five times as great, under similar circumstances.

The relation of clinical to bacteriological diagnosis is a subject also of some interest. It has been claimed again and again, since bacteriological diagnosis was first instituted in various parts of the civilized world, that bacteriologists have discovered diphtheria bacilli in many cases which were clinically innocent, thus raising the total number of diphtheria cases now reported as such by adding in cases which were not, under solely clinical tests, classed as diphtheria at all. This is a favorite argument of the opponents of the use of antitoxin, who maintain that the absolute deaths from diphtheria are now as large, if not larger, than ever, while the admitted reduction in the *percentage* of fatal diphtheria cases these objectors explain as due, not to a lessening in the number of deaths, but to an increase in the total number of cases, the increase consisting largely of the innocent cases discovered, quite needlessly, by the bacteriologists. Such objectors cannot have considered the records on these subjects carefully. As a matter of fact, from the records of three laboratories where the results have been tabulated to show a comparison of clinical and bacteriological diagnoses, quite the contrary is true; for the bacteriological examination is found to really reduce the total number of cases instead of increasing them. In other words, physicians make a clinical diagnosis of diphtheria in innocent

cases more often than a clinical diagnosis of "not diphtheria" in cases really diphtheria, so far at least as laboratory records show. I have prepared a number of tables bringing out these points as follows. The laboratories from which the records come are those of the Pittsburg Board of Health for one year, of the Massachusetts State Board of Health for two years, and of the Boston Board of Health for nine months.

TABLE III.

PITTSBURG BOARD OF HEALTH REPORT, 1897.

*For Year* 1897.

| | | |
|---|---|---|
| Whole number of cases | 897 | |
| Clinical diagnosis, positive: | | |
| Total cases | 561 | 100% |
| Bacteriological diagnosis, positive | 390 | 70 |
| Bacteriological diagnosis, negative | 171 | 30 |
| Clinical diagnosis, negative: | | |
| Total cases | 336 | 100% |
| Bacteriological diagnosis, negative | 274 | 81.5 |
| Bacteriological diagnosis, positive | 62 | 18.5 |
| Actual concordant cases | 74% | |
| Theoretical concordance | 75.7 | |

The clinical diagnosis was negative in 37.3% of all cases where clinical diagnosis was given.

MASSACHUSETTS STATE BOARD OF HEALTH REPORT, 1897.

*For Year ending March* 31, 1897.

| | | |
|---|---|---|
| Whole number of cases | 489 | |
| Clinical diagnosis, positive: | | |
| Total cases | 354 | 100% |
| Bacteriological diagnosis, positive | 239 | 67.5 |
| Bacteriological diagnosis, negative | 115 | 32.5 |
| Clinical diagnosis, negative: | | |
| Total cases | 135 | 100% |
| Bacteriological diagnosis, negative | 108 | 80 |
| Bacteriological diagnosis, positive | 27 | 20 |
| Actual concordance | 71% | |
| Theoretical concordance | 73.7 | |

The clinical diagnosis was negative in 27.2% of all cases where clinical diagnosis was given.

*For Year ending March* 31, 1898.

| | | |
|---|---|---|
| Whole number of cases | 727 | |
| Clinical diagnosis, positive: | | |
| Total cases | 494 | 100% |
| Bacteriological diagnosis, positive | 338 | 69.2 |
| Bacteriological diagnosis, negative | 156 | 30.8 |
| Clinical diagnosis, negative: | | |
| Total cases | 233 | 100% |
| Bacteriological diagnosis, negative | 176 | 75.5 |
| Bacteriological diagnosis, positive | 57 | 24.5 |
| Actual concordance | 70.7% | |
| Theoretical concordance | 72.3 | |

The clinical diagnosis was negative in 32.0% of all cases where clinical diagnosis was given.

Boston Board of Health Report of 1898.

*Nine Months ending Jan.* 31, 1899.

| | | |
|---|---|---|
| Whole number of cases | 765 | |
| Clinical diagnosis, positive: | | |
| Total cases | 219 | 100% |
| Bacteriological diagnosis, positive | 124 | 56.7 |
| Bacteriological diagnosis, negative | 95 | 43.3 |
| Clinical diagnosis, negative: | | |
| Total cases | 546 | 100% |
| Bacteriological diagnosis, negative | 488 | 89.6 |
| Bacteriological diagnosis, positive | 58 | 10.4 |
| Actual concordance | 80% | |
| Theoretical concordance | 73.1 | |

The clinical diagnosis was negative in 71.4% of all cases where clinical diagnosis was given.

The theoretical concordance given above is the concordance estimated from the results tabulated which would be obtained as the actual concordance, had the number of positive clinical diagnoses equalled the number of negative clinical diagnoses.

SUMMARY.

| | |
|---|---|
| Total cases reported as diphtheria on bacteriological diagnosis, but clinically "not diphtheria" at time of taking culture . . . . . . . . . | 204 |
| Total cases reported as "not diphtheria" on bacteriological diagnosis, but clinically diphtheria at time of taking culture . . . . . . . . . . | 537 |

In other words, the bacteriologist added 204 cases to the total, but at the same time subtracted more than twice as many. Thus for these three laboratories the full record stands thus: —

| | | |
|---|---|---|
| Total cases reported positive clinically and examined bacteriologically . . . . . . . . . . . . . . . . . | 1,628 | 100% |
| Total cases found positive bacteriologically . . . . . . | 1,295 | 79.5 |
| Total reduction in diphtheria cases due to bacteriological examination, so far as can be calculated from comparison of clinical and bacteriological diagnoses . . . . | 333 | 20.5% |

Hence it will be seen that a little over 20 per cent. of the cases which physicians set down distinctly and positively as diphtheria, from the clinical symptoms, were shown not to be diphtheria by the bacteriological diagnosis in these three laboratories. It must be remembered, of course, that this does not represent all the cases examined bacteriologically by any means; for in a considerable number of cases a comparison of clinical and bacteriological examination cannot be made, because the clinical diagnosis is doubtful or "not given." Nor do all the cases examined bacteriologically form more than a small part of all the cases reported as diphtheria. Many reported cases throughout this country never pass through a bacteriological test at all, although in Boston itself the present regulations provide that every case, except those terminating fatally early in the disease, shall so pass at some time or other during its course. Nevertheless, judging from the cases which do go through such a test, it is fair to assume that, if every case reported as diphtheria by physicians were thus tested for diagnosis early in the attack, then the total number of diphtheria cases would be reduced, although probably in a smaller proportion. It is especially interesting to note that Boston physicians send in for examination an extremely large number of cases which they consider clinically innocent. Thus, while the records of the other laboratories under consideration show from 27 to

37 per cent. of negative clinical diagnoses, the Boston Board of Health shows no less than 71 per cent. In Boston the reduction in reported cases, due to bacteriological examination alone, is 40 per cent. of cases examined. If, however, for each of these laboratories calculations be made showing the concordance and discordance of clinical and bacteriological results, supposing the number of clinically negative and clinically positive cases to be equal, a remarkable uniformity of results is found to exist in these three laboratories. Thus it is found that about 73 to 75 per cent. of clinical diagnoses are confirmed, about 25 to 27 per cent. are reversed, in each.

The relation of bacteriological results to the presence or absence of membrane in cases for diagnosis is another point over which a good deal of discussion has occurred. It has not infrequently been held that the bacteriologist often finds bacilli present, not only when clinical symptoms point to innocence, but even in the absence of membrane or anything that might be mistaken for membrane. The following table will answer this question : —

TABLE IV.

| *Bacteriological Diagnosis.* | *Positive.* | *Negative.* | *Totals.* |
|---|---|---|---|
| Membrane present . . . . . . . . . . . . . | 252 | 588 | 840 |
| Membrane absent . . . . . . . . . . . . . | 17 | 356 | 373 |
| | 269 | 944 | 1,213 |

Same table reproduced, in percentages : —

| *Bacteriological Diagnosis.* | *Positive.* | *Negative.* |
|---|---|---|
| Membrane present . . . . . . . . . . . . . | 93.6% | 62.3% |
| Membrane absent . . . . . . . . . . . . . | 6.4 | 37.7 |
| | 100% | 100% |

It will be seen that the 17 cases positive without membrane form less than 2 per cent. of the total 1,213 cases examined, and that more than 60 per cent. of the negative cases possessed a membrane or something like one. Of course, it is impossible to say how many of this 60 per cent. presented a real membrane and how many simply an exudate which was entered as membrane. More than 93 per cent. of the cases in which diphtheria bacilli were found showed membrane, however. The 17 cases in which no membrane existed

at the time of taking the culture had symptoms or other clinical reasons pointing to the possibility of infection.

The question of infection by exposure without any clinical symptoms or subsequent development of the disease which follows naturally here is rather a difficult one to answer definitely from our records, because of the small number of such cases sent in. Our records show 35 cases simply exposed to diphtheria, the cultures from which show 1 positive result, 33 negative, and 1 no growth. These were cases in ordinary private city practice. In institutions a different condition seems to hold good. For instance, in one home for children in Boston, wholesale cultures were made on the appearance of one or two cases. These cultures were repeated at frequent intervals. Only four positive cultures were obtained on the first examination, but a total of 40 positive cases were detected during the course of the whole investigation. I could not get exact records, but it would seem that many of these positive cases never developed clinical symptoms at all.

The length of time which bacilli persist in the throat is another subject often discussed. Here also the impression seems to exist that bacteriological examinations give trouble by prolonging unnecessarily a large number of cases for considerable periods of time. But a consideration of the figures obtained in practical work shows that this impression is mistaken. Thus the first final negative is obtained within twenty days from the date of earliest symptoms by us, in about 40 per cent. of cases which yield sufficiently complete data for tabulation. About 60 per cent. yield the first final negative before the twenty-fifth day. 74 per cent. of the cases show a first final negative within thirty days, about 86 per cent. within thirty-five days. Thirteen per cent. lasted from thirty-five to sixty-five days. We had no cases lasting longer than this. The average bacteriological history of an ordinary case is this : —

TABLE V.

| | *Average.* |
|---|---|
| From earliest symptoms to first positive culture . . . . . . . . | 2.5 days |
| From first positive culture to last positive culture . . . . . . . . | 18.8 " |
| From last positive culture to first final negative . . . . . . . . . | 4.5 " |
| From first final negative to second final negative . . . . . . . . | 2.7 " |
| Total . . . . . . . . . . . . . . . . . . . . | 28.5 days |

Thus the bacilli are found on an average for 21.3 days only from the earliest symptoms. Nevertheless, it would obviously be impossible to make an arbitrary time limit of, say, thirty days during which isolation should be enforced, and at the end of which release should be granted without bacteriological release; for such a time limit would prolong the isolation unnecessarily of about 40 per cent. of cases for ten days each, while, on the other hand, it would permit the release of about 26 per cent. of cases while still infective. Evidently, the release by bacteriological culture is then really not only a protection to the public health, but also a very considerable time-saver. Should twenty-eight days be selected as the arbitrary time limit, 40 per cent. of cases would receive at least five days' unnecessary isolation, whereas another 40 per cent. would be released while still infective.

To sum up the evidence so far available, it would appear: —

*First.*— That single cultures may be relied upon to settle the question of diagnosis in from 90 to 95 per cent. of cases.

*Second.*— In from 5 to 10 per cent. of cases a single negative culture for diagnosis is unreliable, so that, if the clinical evidence points to the presence of diphtheria, the diagnosis of diphtheria should not be given up unless two negatives at least are obtained.

*Third.*— In about 28 to 30 per cent. of positive cases one negative culture for release may be obtained while the bacilli are nevertheless still present; but two consecutive negative cultures for release will not allow more than 3 per cent. of such cases to escape detection.*

*Fourth.*— Bacteriological examinations tend: (1) to reduce the total number of cases finally diagnosed and reported as diphtheria, while making the actual diagnosis more correct; (2) to reduce the total time of isolation, while giving to each case the proper length of time, short or long, as may be necessary in each case.

*Fifth.*— The working error of bacteriological examinations for diagnosis is in positive *cases* about 5 to 10 per cent., if one culture be trusted to in each case. If two cultures be taken, the working error is practically nothing. In negative *cases* the working error also is practically nothing. Clinical diagnoses show a working error of from 10 to 40 per cent. Finally, while bacteriological diag-

* From records of the Boston City Hospital, where three negative cultures are required.

nosis is not perfect in its results, it affords the one definite method for approximating to accuracy and precision, it constitutes a court of appeal of great value, not only from a scientific standpoint, but also in the handling of obstructionist patients, both in private practice and in public health work, while, intelligently considered and carried out, it relieves those who employ it of a large share of responsibility in one of those diseases in which responsibility is particularly heavy, and clinical evidence often unreliable.

The President.— Dr. Hill's paper is now before you for discussion or question.

Dr. Chapin (of Providence).— Our experience in Providence in examining cases of diphtheria has, in most respects, been the same as that narrated by Dr. Hill; and in those points in which it differs I think there is a good explanation for the difference, perhaps. I should like to ask Dr. Hill, however, if I am mistaken in assuming that in Boston it is the custom to take a double culture in each case, one from the throat and one from the nose.

Dr. Hill.— It is not at the present time, Dr. Chapin. The best we can do, apparently, in practical release work, is to insist on two negative cultures from whatever was the seat of lesion. We have not yet reached the point where we can enforce a regulation requiring release cultures from both nose and throat in every case, as we should. We recognize the sanitary value of the proceeding, and that it ought to be done; but, practically, it has never come to the point yet where we could do it.

Dr. Chapin (of Providence).— Is it not the custom to do it in the primary culture?

Dr. Hill.— No. In primary cultures the board of health has no control. The doctor need not take one at all unless he likes. When he does take it, he can take it from anywhere that he pleases.

Dr. Chapin (of Providence).— I am very sorry, then, that my explanation of the difference in results is not what I imagined it was. In Providence our preliminary cultures are not so reliable as Dr. Hill states here. I cannot give you any of the figures now, because I carelessly forgot to bring a report with me; but, certainly, the first culture, even when the person is sick, cannot be trusted in 92 per

cent. of the cases. I imagined it was because it was customary in Boston to take two cultures, one from the nose and one from the throat, as I know it was some years ago, when the use of cultures was first introduced; and that has not been done in Providence, but a culture is made from the throat alone. I think, then, the unreliability of our first cultures must be due to the method of taking them, which is by means of a swab instead of a wire, as I believe you use here. I should dislike very much to think it was due to defects in the bacteriologist; and I can hardly think that it is so, because we have had several men examine the cultures in Providence, and they certainly all agree pretty well in regard to the ratio of unreliability of a single culture. We cannot get it right in 92 per cent. of the cases, as is done here.

In regard to the culture method decreasing the number of cases of diphtheria, it does not do that in Providence. It has increased the number of cases of diphtheria; and I think the reason for that is the very great efforts which have been made by the Health Department to secure cultures from every case of sore throat, no matter what it is. There are a great many physicians in Providence who, as a routine, take a culture from every case of sore throat they have. And we have made a very great effort, in looking up our cases, to look after those cases of tonsillitis next door that Dr. Hill tells about; and we are getting hold of all of the cases in the neighborhood, if possible, and have cultures taken from them. In a great many cases they do not show that the diphtheria bacillus is present. On the other hand, we often find a case of diphtheria in this way, a case which was spreading it perhaps in the school or elsewhere. I think that if every case of sore throat in Boston could be examined, or as large a proportion of cases as is done in Providence, you would have a great many more cases which could properly be called diphtheria. As a matter of fact, in Providence in 1896 the mortality of persons who were sick with diphtheria, and who did not have antitoxin, was 13.9 per cent.,— a decided reduction from the mortality of 27.41 of 1894, when there were neither antitoxin nor cultures, a reduction due not to the use of antitoxin, but to the use of the culture method of diagnosis.

In regard to the presence of the diphtheria bacilli in persons who have been exposed to the disease, and yet who have no membrane in the throat and who, perhaps, do not have any apparent inflammation of the throat at all, we have a large amount of experience, because for over two years we have insisted upon a bacteriological examination of the throat of each person in a family wherever there is a case of Klebs-Löffler diphtheria discovered, so that we have a record of many hundreds of cases of well persons whose throats have been examined,— persons who have been undoubtedly exposed to the disease, — and in a large proportion the diphtheria bacilli is found. In the two years 1897 and 1898 the proportion was 17.8 per cent. in adults, and 23.2 per cent. in children. The number examined was 1,522. We make it a practice in Providence, if possible,— and usually it is possible,— to maintain isolation in all such cases. Unfortunately, we have not been able to do what seems to me to be very important; and that is, to test the virulence of the bacilli in these cases. We have examined some of them, but not very many. My impression is that we would find that the bacilli in a considerable portion of the throats were not virulent. We have had just the same experience in institutions that Dr. Hill reports,— that, when diphtheria gets into an institution, and we can control all the patients and examine all throats and examine them repeatedly, we usually find a good many cases where the bacilli are present; and it often remains for some length of time, though it has been our experience in Providence that, if there are no clinical symptoms, the bacilli do not remain as long as when the clinical symptoms are present.

Another investigation which we have made is the examination of throats of persons who have not supposedly been recently exposed to diphtheria and who are perfectly well. We have had examined several hundred such cases within the last six or eight months, and in quite a number of them the bacilli were found present. In none of them that we have tested have the germs been shown to be virulent. But I will report one quite interesting case. Over a year ago, in an orphan asylum with a couple of hundred children, we had a diphtheria outbreak, and one death and several sick, and twenty-five or thirty cases of children not sick, but with the bacilli persisting

for some length of time. We got a negative culture from each person in the institution before removing the placard. About nine or ten months afterward I went there, and had a culture taken from every child in the institution. One of them showed the presence of the Klebs-Löffler bacillus. A short time afterward anothei one was reported as sick with the disease, with membrane upon the throat. The bacilli were found present in that case; and a culture was made from every other child in the institution, and no diphtheria bacilli were found to be present. What we have done in Providence leads us to think that the Klebs-Löffler bacillus as demonstrated upon the culture medium is extremely common in those that are exposed to the disease in the family, and that it is, also, not very rare in the community at large. Now a practical question comes. Shall we isolate every person in whom these bacilli are found? If not, how can we draw the line? If we are going to use the bacillus at all as a means of diagnosis for diphtheria, it seems to me the only logical thing for us to do is to isolate in every case where it is found.

Or there is another thing we can do. There are certainly a great many cases where the germ is not virulent when injected into guinea pigs. Now comes the second practical question. Shall we in every case examine the organism for virulence? That certainly would be very laborious. Or shall we rather test it in certain cases,— cases which have been running on an unduly long time or cases where it would produce very great hardship to maintain isolation? There is also another practical question which I should like to have Dr. Hill answer. In fact, I can ask him a great many questions. I should like to know if we are at all positive that there is any close relationship between the virulence of the organism as tested upon a guinea pig and its virulence when existing in the throat of a human being.

Dr. Hill.— I may be pardoned for correcting one misapprehension on the doctor's part. We have abandoned the platinum wire, and use swabs instead; and they appear to be very satisfactory. I have never had any experience with the platinum wire myself outside of the laboratory. I can only take as evidence what is told me by various physicians; but they seem to agree that it is very much more satisfactory to use the swab than the wire, so that I think that

that source of difference between our results will have to be ruled out.

With regard to the virulence of the organism found in the throat a good deal of work has been done. We have not done much of it in Boston: the question has never come up very strongly there. The Massachusetts State Board of Health did some very good work upon that point some two or three years ago, and so also have a number of other laboratories, so that it seems to be pretty well established that certainly 75 per cent., at least, of cases contain bacilli which are virulent even after membrane disappears. In Brooklyn, N.Y., up to 1898, under Wilson, who then had charge of the work, a time limit was set, after which every case was tested. If the bacilli lasted in the throat longer than four weeks, the bacilli were tested for virulence. His results were 77 per cent. "and over." I suppose the "over" refers to some cases in which they had laboratory accidents, etc.; but at least 77 per cent. gave virulence. Of course, in testing virulence there are a good many questions to be decided. In the first place, if a number of single colonies be tested, each of which represents a single bacillus in the throat, it will be found that some of those are virulent and some not at all, so that it is necessary in making the test to try a number of colonies rather than only one. That is, of course, a matter of technique.

Again, the dose which is given to the guinea pig is to be considered; and also on just what lesions the decision as to virulence is to be based is a very important question. Of course, if you expect the bacilli always to kill the guinea pig before you call them virulent, you may get a much lower rate of virulence than if you accept the formation of local lesion of a greater or less extent as sufficient evidence. In that way a good many discrepant results could be explained. But Dr. Theobald Smith's work in that line was very excellent, because he did not make the test by injecting the animals with cultures, but with toxin prepared from cultures; and he standardized this toxin in every case. In those cases which he completed, very little, practically no difference in virulence was observed in the bacilli obtained early in the disease and late in the disease. As to the relation of virulence in the human throat to virulence in the guinea

pig, it is a very difficult matter to settle. It is almost impossible to make direct inoculations, comparative inoculations, from one human patient to another, to compare with parallel guinea pig inoculations! If we could do that, we could settle that question very readily; but the clinical evidence of the infection from one case to another is usually so loose and so indefinite that it is usually impossible to base anything definite on it. It might be said just in this connection that Park's bacillus No. 8, which supplied large sections of this country for a long time with very virulent toxin for the manufacture of antitoxin, was isolated, I believe, from a particularly mild case.

With regard to the diphtheria occurring in institutions. Being in Canada within the last month or so for a day or two, the bacteriologist of the Provincial Board there told me he had been going through some institutions, taking cultures as the cases came in from every throat indiscriminately, and then at intervals taking cultures again from everybody in the institution. He found that a very few of those cases which came to the institution had the bacilli in their throats when they entered. The exact figures I have forgotten, but it was extremely small or practically nothing. On the other hand, although there were no diphtheria cases in the building, yet a few weeks after entrance the cultures taken from the inmates would show in a certain proportion of cases diphtheria bacilli, so that it seemed that in some cases, at least, institutions might become infected with bacilli in some way or other which very rarely produced typical clinical cases, while, nevertheless, the bacilli could be found on cultural examination. I think the question of institutional diphtheria will have to be discussed on an entirely different basis from the diphtheria of private families in private practice. I think that opinion is growing throughout the bacteriological world, and that we shall have to separate the statistics relating to institutions from those relating to private practice as well as from those obtained in hospitals. It is sometimes a little difficult to get the latter. Institutions in which an epidemic occurs may very often be in rather a rush, and may not feel like keeping careful records of every culture and of the condition of each patient, so that the bacteriologist is rather neglected after he sends in his report positive or negative, making it difficult to get definite data.

In all our records I have carefully excluded such institutions as we had, because what I wanted to demonstrate was the results to be obtained in actual house to house practice throughout the city of Boston, so far as we handle it.

Dr. Gage.— I should like to ask Dr. Hill if his records show in what percentage of the cases found to be positively diphtheria by culture antitoxin was used, Mr. Chairman.

Dr. Hill.— No, sir, I cannot answer that question. We don't handle antitoxin at all: the State does it, so we have no record. However, I know that it is used in a very large proportion of cases. Physicians often come and ask for a diagnosis, so that they can influence the patients to let them use antitoxin where the patients object. In this way our diagnoses are often helpful.

Dr. Durgin.— I should like to say, in regard to the delay in the use of antitoxin for any purpose, that it is a very serious fault. The case is suspected of being diphtheria, the culture is taken, and delay is occasioned in getting the "positive" before antitoxin is given. It seems to me that we have advanced far enough in the use of antitoxin in the history of this dreaded disease, and especially as to its fatality without antitoxin, to warrant us in advising that just as soon as a suspicion is aroused of its being diphtheria the advantage given by antitoxin should be utilized. Cases come to my knowledge frequently where fatal results occur before antitoxin has been used and before the "positive" from the bacteriologist has been received. Inasmuch as we feel fairly certain that no injury will be produced by giving the antitoxin, it should be given whether you are certain that the suspicious case is going to turn out diphtheria or not.

Dr. Miller.— Mr. President, I should like to ask Dr. Durgin just one question. There are some cases in the country where we have follicular tonsillitis that is taken for diphtheria. In case antitoxin was used in those cases, what would be the effect? Would it be any advantage or any disadvantage? Suppose it was not diphtheria, but was supposed to be and it proved to be follicular tonsillitis, and antitoxin was used, would there be any particular effect upon the tonsillitis?

Dr. Durgin.— From all that we have learned concerning the effect of antitoxin, you will do your patient no injury whatever by giving antitoxin. You may save the patient's life by giving it early.

Dr. Chapin (of Springfield).— The last case of diphtheria I had the bacteriologist declined to find any bacilli in after three examinations, and I was obliged to say that the patient never had diphtheria. It was as good a case of clinical diphtheria as I ever saw, with a firm patch on the posterior and lateral wall of the pharynx, increasing on the following morning to a small patch on the other side, yielding after the administration of antitoxin precisely as I have so many times seen true diphtheria membrane disappear after the use of antitoxin. I could not help feeling as if the antitoxin had treated the streptococcus which was working there very much as it would have treated the Klebs-Löffler. It may be all moonshine, but that is the way it looked.

Dr. Gage.— I did not know, Mr. President, that the question of the use of antitoxin was going to be raised. As long as Dr. Durgin has seen fit to speak of it and as long as here in Boston we never hear anybody — at least, I never have — raise any opposition to the use of antitoxin, I should like just to state a reply that I got from a respected member of our profession a short time ago, when I was holding a discussion with him in regard to the use of antitoxin. Perhaps Dr. Durgin will reply to him. We talked some time. He is a man who received his education long before the days of bacilli and antitoxin, has conducted a useful and successful and honorable practice, is a man who is mentally still awake; and yet I knew that he did not very often take a culture to the bacteriologist, and I was quite sure that he very seldom used antitoxin at all. In a little discussion with him about it I brought out this from him. This was his reply: No, he said, he did not very often use it. He said he was not convinced yet, he was not thoroughly convinced, that it was the thing to use. I asked him for his reason, and this was his reply: "You say that the deaths from diphtheria after the use of antitoxin are reduced from between 30 and 40 or even over 40 to perhaps 12 per cent. If that is true, then if you take the period since the use of antitoxin, say in the State of Massachusetts, for the five or six

years since it has been in use, you ought to find a very marked diminution in the total number of deaths in this State as compared with the equal period preceding it." He claimed the figures did not show it, and I could not find that they did.

The President.— I certainly can answer for that, that the figures do show it.

Dr. Gage.— The total number of deaths in Massachusetts?

The President.— Yes.

Dr. Gage.— He brought up his figures. I am very glad to be able to answer him now. I did not have the figures at hand.

Dr. Durgin.— The diminution in Boston alone has been from some 700 or 800 deaths a year down to less than 200.

The President.— It seems to me that Boston has furnished in this matter what it has furnished in so many other matters of preventive medicine, results of the very highest importance. The south department of the city of Boston, in its hospital, which is by all odds the best hospital for the control and treatment of infectious diseases, communicable diseases, that I know of anywhere,— certainly, the very best in this country,— is under the active administration of a man who believes in what seemed to me absolutely unnecessarily high doses of antitoxin. It seems to me that Dr. McCollom uses antitoxin more recklessly than any human being on the face of the earth. I say that because the State furnishes it. But there is no evidence whatever in the records of the hospital that I have ever been able to find that that use has been mischievous, injurious, to a single patient. The record is a wonderfully good one. In talking with Dr. McCollom on the subject, I criticise, as I say, this enormous use of it; and yet when he says to me, "I know perfectly well that I differ from any other authority perhaps in the world in this matter, but, then, my patients get well," there is absolutely no answer to that.

Dr. Chapin (of Springfield).— How much does he give, Doctor?

The President.— Oh, two, five thousand units without any hesitation,— twenty-five thousand in one case.

Is there anything else to be said upon this subject? It seems to me the afternoon has been a singularly productive one and a singu-

larly interesting one. I think we have to thank Dr. Hill again, as we did Dr. Richardson, for his contribution. Is there anything else to come before the Association at this time? If not, it will be quite proper that we adjourn, I presume, if any one will make a motion to that effect.

A motion to adjourn was made.

The President.— Dr. Durgin makes a motion that we adjourn, to meet at Gallop's Island in July. If that be your pleasure, you will signify it by saying, Aye: contrary-minded, No; and it is a vote.

Adjourned.

# JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH

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## July Meeting, 1899

SUBJECT: The Incubation and Infective Periods of Some of the Infectious Diseases.

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# THE JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

The Massachusetts Association of Boards of Health was organized in Boston in March, 1890, with the following objects: the advancement of sanitary science in the Commonwealth of Massachusetts; the promotion of better organization and co-operation in the local Boards of Health; the uniform enforcement of sanitary laws and regulations; and the establishment of pleasant social relations among the members of the Association.

All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

The Official Journal of the Association is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them. No part of this matter is printed in any other periodical.

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# MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

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[The Association as a body is not responsible for statements or opinions of any of its members.]

VOL. IX. October, 1899. NO. 3.

## JULY MEETING

OF THE

## Massachusetts Association of Boards of Health.

The quarterly meeting of the Association was held at Gallop's Island, Boston Harbor, on the afternoon of July 27, 1899, the President of the Association in the chair. Following is a report of the proceedings.

THE PRESIDENT.—The first business is the consideration of the following names presented as candidates for membership in this Association: —

| | |
|---|---|
| SILAS H. AYER, M.D. | Boston. |
| JOHN F. WORCESTER, M.D. | Clinton. |
| HARRY A. STONE | Newton. |
| H. D. CHADWICK, M.D. | Waltham. |
| GEORGE S. FULLER, D.V.S. | Lawrence. |
| W. F. HARBACH | Newton. |
| HERBERT C. EMERSON, M.D. | Springfield. |
| H. L. COOPER | Weston. |
| GEORGE ABERCROMBIE | Weston. |

Is it your pleasure that these gentlemen be elected members of this Association? If so, you will signify it by saying ay: contrary-minded, no; and they are duly elected.

As to the next place of meeting of the Association, a very strong

representation has been made in favor of a meeting at Leominster. What is the feeling of the Association with regard to that location?

Dr. Durgin.—I move the meeting be held in Leominster in October.

The motion was seconded and adopted.

The President.—Two other names have been presented by the Executive Committee for membership in the Association. H. L. Cooper and George Abercrombie, of Weston. Is it your pleasure that these gentlemen be elected to membership?

The two gentlemen named were elected to membership in the Association.

The President.—Is there any other incidental business to come before the Association at this time? If not, we will proceed to the regular business of the meeting.

The first paper is upon "The Incubation and Infective Periods of Some of the Infectious Diseases," and the Association is very fortunately going to listen to Dr. Cutler on the subject; and Dr. Cutler I have now the pleasure of introducing to you.

## THE INCUBATION AND INFECTIVE PERIODS OF SOME OF THE INFECTIOUS DISEASES.

BY ELBRIDGE G. CUTLER, M.D.

The Incubation Period is the time elapsing between the establishment of the disease in the body, which usually dates from the exposure of a susceptible individual, and the onset of characteristic symptoms. It is liable to variation from the size of the "dose" * of

* Ballard (Report of Committee appointed by Clinical Society of London, 1892, to investigate periods of incubation and contagion of certain contagious diseases) found in one typhoid epidemic 43 cases infected from milk. Of these, 19 used the milk freely as an article of diet, and all but 4 of them applied for medical aid within the first fortnight of the outbreak. 24 of the persons attacked used very little milk; and 17 of these sought medical aid in the third and fourth week of the outbreak, 3 only in the first, and 5 in the second week. Power (same report) records a case where typhoid followed a single draught of infected milk in three weeks, whereas large milk-drinkers in same outbreak suffered early.

the infective agent, its virulence, and the resistance of the individual to its encroachment.*

The incubation period determines the length of time which a susceptible individual who has been exposed to infection should be kept under observation before he can be said to have escaped.

The Period of Isolation is the time during which it is advisable to keep the patient apart from others.

In the preparation of this paper it has been thought worth while to consider only those diseases especially which concern us in this locality; and, as a consequence, Relapsing Fever, Dengue, Asiatic Cholera, and Bubonic Plague have been omitted from consideration.

## TYPHOID FEVER.

### *Incubation.*

Murchison concludes as follows: 1. The period of incubation of typhoid fever is most commonly above two weeks; 2. Instances of a longer duration are more common than in typhus or relapsing fever; 3. It is often less than two weeks and may not exceed one or two days.

Liebermeister considers the average period of incubation as three weeks.

Professor Quincke (*Correspondenz-Blatt für schweizer Aerzte*, 1875, No. 8) says, in a series of accurately observed cases, the shortest period was eight days, the longest certain period between sixteen and eighteen days.

In the Marylebone milk epidemic of 1873 a child was taken ill five days after drinking the infected milk (Cayley).

Hilton Fagge says that it has been conjectured that incubation is shorter when the poison is inhaled with the breath, longer when it is swallowed in drinking water.

During this latent period the patient may feel quite well, or, on the

* Chantamesse asserts, on the authority of Dawson Williams, that certain individua's retain for a long time in their intestines, or perhaps even in the substance of their tissues, typhoid germs, which develop ill until the occurrence of some favoring circumstance. A similar retention of the infective agent in a condition of quiescence for long periods is not very infrequent in diphtheria, pneumonia, and suppurative diseases until some general deterioration of health or of the local power of resistance enables it to become established, grow, and produce characteristic effects. Washbourn, Proceedings of the Royal Medical and Chirurgical Society, London. Meeting of Nov. 26, 1895.

other hand, may complain of being out of sorts. There may be languor or tendency to diarrhœa and vague feelings of discomfort and chilliness, with headache and anorexia.

Wilson (American System of Practical Medicine, Loomis, vol. i. p. 179, 1897) says this period is variable. In general, it may be set down as from two to three weeks. In some well-authenticated cases it has not exceeded four or five days. He mentions a case of only four days' incubation.

Williams (Twentieth Century Practice of Medicine, vol. xiii. p. 381) says the determination of the incubation period of typhoid fever has always presented much difficulty, owing to the paucity of cases affording evidence on the point and the difficulty of settling exactly the date at which an attack begins. During the period of incubation the patient is apt to suffer from various ill-defined symptoms, and this condition merges gradually into a developed attack. In other cases, however, the onset of the disease is sudden; and the Report of the Clinical Society, based on analyses of reports of outbreaks down to 1890, has provided a considerable mass of information which is found to confirm the opinions of the best authorities published previously to that date. Their conclusion is: "The interval between exposure to infection and the development of distinct symptoms is probably most often twelve to fourteen days. It is not very infrequently nine or ten days, occasionally eight, and possibly less, In rare cases it is prolonged to fifteen, eighteen, or even twenty-three days." There is some reason to believe that, when the "dose" of the infection is large, the incubation is short. See above notes.

With regard to the Period of Observation, in the majority of instances, in epidemics at least, the infection is derived indirectly from the patient through the medium of water or milk. Experience shows that an epidemic due to contamination of milk with the typhoid bacillus will come to an end at or about the end of the second week after the supply of the infected milk for consumption has been stopped. When water is the distributing medium, a much longer time may elapse before cases cease to appear. Thus, when a public water supply has been contaminated, cases have often continued to occur until the fourth week after the source of the specific pollution has

been removed; and in cases of well-water the length of time is apt to be very much greater, owing probably to the fouling of the soil and the difficulty of insuring thorough disinfection. The period of observation, therefore, is somewhat uncertain; and, if the water supply cannot be entirely changed, it should extend over a month at least.

A patient suffering from typhoid fever is capable of transmitting it to others throughout the entire course of his illness, from the date of the earliest symptoms down to the end of the second week of convalescence at least; and his capability returns during a relapse. Infection can be conveyed by fomites, and retained by them for two months at least. Washerwomen have frequently been infected by washing the linen of patients the nature of whose illness had not been recognized at first, so that no disinfection had been practised. Richardson * found typhoid bacilli in the urine of a number of cases of typhoid. They were apt to appear in the later stages of the disease, and persisted in the great majority of cases far into convalescence.

Brannan (Twentieth Century Practice of Medicine, vol. xvi. p. 615) says the usual duration of incubation period is from eight to fourteen days. In rare instances it may not exceed one or two days. Still more rarely, it may extend three or even four weeks.

*Conclusion.*

1. The Period of Incubation of Typhoid Fever is most often twelve to fourteen days, frequently nine or ten days, occasionally eight and possibly less. In rare cases it is prolonged to fifteen, eighteen, or even twenty-three days.

2. The Period of Observation is uncertain, and under some circumstances should extend over twenty-eight days; namely, where the water supply cannot be changed.

3. The Period of Isolation, in the ordinary acceptation of the term, should extend through the period of convalescence; and proper disinfection of the stools and urine should be practised for at least a month after the symptoms have ended.

* Bacteriological Studies upon the Urines of Persons Sick with Typhoid Fever. Mark W. Richardson, Massachusetts General Hospital, 1898.

## MALARIAL FEVER.

The Period of Incubation most commonly observed is ten to fifteen days, the extremes being probably six and twenty-one days. The shorter the interval between the paroxysms in the fever when it first develops, the shorter also, probably, the period of incubation. Thus, in tertian, it is shorter than in quartan. When induced by injection of malarious blood into healthy persons, the disease has occurred at periods varying from six to sixteen days. (Williams, Twentieth Century Practice of Medicine, vol. xiii. p. 385.)

## TYPHUS FEVER.

### *Incubation.*

Moore says, "In typhus fever there seems to be no fixed duration for this stage." In a large number of cases it is about twelve days. It rarely, if ever, exceeds three weeks. (Allbutt's System of Medicine, vol. ii. p. 356.)

Murchison (St. Thomas Hospital Reports, 1871) collected 31 cases, and gives the following conclusions: —

1. The period of incubation of typhus fever varies in duration in different cases.

2. In large proportion of cases it is about twelve days.

3. In exceptional cases it is longer than twelve days; but it rarely, if ever, exceeds three weeks.

4. In many cases, one-third or more, it is less than twelve days; and occasionally there is scarcely any latent period, the symptoms commencing almost at the instant of exposure to the poison. It would seem that the poison of typhus may be so concentrated or the system may be so susceptible to its action that its effect may be almost instantaneous. Generally, the patients are conscious of the moment when the fever poison enters the system.

Moore says he has often suffered from a "typhus headache" almost immediately after exposure to the poison of this disease; and the same thing is said by Doty, of New York.

Moore gives in detail the history of the case of Dr. Harvey, of Dublin, who died Dec. 28, 1881, of typhus fever, in which the period of

incubation could not have exceeded ten days, and was, most probably, eight or nine days in length.

Niemeyer (Text-book of Clinical Medicine) gives two examples of typhus fever which are valuable as contributions to the evidence as to the duration of the period of incubation.

In the year 1854, 2 typhus patients were received into the Magdeburg Hospital from the prison, which was much crowded. For months previous there had been only a few cases of typhoid, and not a single case of typhus in his wards. Eight days after the reception of these patients, two others, who had lain beside them, were attacked by the same disease. One of the latter patients had been admitted for intermittent fever, and the other for epilepsy.

Again, in March, 1855, a tradesman from Heiligenstadt was attacked with typhus fever while away from home. He was received into Niemeyer's ward, in which no typhus had been treated for almost a year. Eight days after the reception of these patients a blacksmith's apprentice and a mechanic lying next to him were attacked by the fever.

Murchison suffered from two attacks of typhus. In the second the incubation stage lasted exactly five days.

Davies (in the *Medical Times and Gazette* of Oct. 19, 1867, p. 428) says 4 Norwegian sailors, on the night of the arrival of their ship in Bristol from Onega, visited some typhus fever nests; and all 4 sickened with typhus eight days later.

Doty (American System of Practical Medicine, 1897, vol. i. p. 340) gives the range as from eight to twelve days, usually the latter. Hutchinson, of Philadelphia, refers to a case where the incubation was thirty-one days. In the city of New York, where a clear history could be obtained in the cases of typhus occurring in 1881–82 and 1892–93, the period of incubation was found to be about twelve days.

Weiss (Diagnostisches Lexikon, 1893) gives the period of incubation as two to three days.

Williams (Twentieth Century Practice of Medicine, vol. xiii. p. 380) says the incubation period of typhus fever is usually twelve days, but shorter periods are by no means rare, and many well-authenticated cases of periods of eight days are on record. The longest known period of incubation appears to be twenty-one days, but instances of periods approaching this length are rare.

The Period of Observation should be twenty-one days; and the patient may be discharged, if in good health, at the end of this time.

The infection of typhus is very virulent, but the area in which it acts is limited.

Murchison believed it was most infectious "from the end of the first week up to convalescence." During the first week of the fever there is relatively little danger; and, if the patient is isolated within that time, others seldom contract the disease. Infection ceases when the appetite and digestion are restored. The average duration of an attack of typhus is about two weeks (13.43 days); and, of 500 cases that recovered, in not one did the duration of the illness exceed nineteen days. Three weeks would therefore appear to be sufficient time for isolation. The infection clings to clothes so much that it has been supposed that typhus fever is most infectious during convalescence, the real fact (Williams) being that the imfection in the cases on which this opinion was founded was derived from clothes which the convalescent then again began to use. Clothes worn at or about the time of the beginning of the disease, and bedding used during the attack, should therefore be disinfected or burned.

Licéaga (Twentieth Century Practice of Medicine, vol. xv, p. 275) gives the average duration of incubation as twelve days, It may be less than three or as many as twenty.

Clothing may retain the poison for long periods. He narates a case where underclothing used by a patient was washed and ironed, but not disinfected, and put away for eight months, and then worn by two sisters, who were taken ill with the disease. The germ of typhus fever may thus retain its activity to at least eight months.

*Conclusions.*

1. The Incubation Period of Typhus Fever is usually twelve days, but it may be as short as eight or as long as twenty-one days.

2. The Period of Observation after exposure should be twenty-one days.

3. The Period of Isolation should be twenty-one days; and thorough disinfection of the patient and his belongings, and burning of articles where possible, should be practised before release of patient.

## MUMPS.

### *Incubation.*

The Period of Incubation of mumps is a somewhat variable one, and different estimates of its length are given. In general, it would seem to vary from one to three weeks, with an average of about two weeks. (Griffith, American System of Practical Medicine, Loomis, 1897, vol. i. p. 726.)

An incubation period of six weeks has been reported (Nicholson) and as short as three or four days (Leitzen). There are, as a rule, no symptoms during this time.

Eustace Smith (Allbutt's System Practical Medicine, 1897, vol. xi. p. 233) gives the report of the Clinical Society (Supplement to vol. xxv., 1892) in which mumps incubation was said to vary from fourteen to twenty-five days. When the complaint begins, it is infectious from the very first, and can be communicated while the patient is merely ailing, and before any signs of glandular swelling are to be detected. Its duration is ten days to a fortnight, but the patient must be considered unsafe to others for at least a week longer.

Kauders (Diagnostisches Lexikon, 1894, p. 621) gives the incubation stage as from nine to twenty-five days. The contagion is usually over at the end of three weeks.

Williams (Twentieth Century Practice of Medicine, vol. xiii. p. 379) says the incubation period in epidemic parotitis is long, the usual period being twenty-one days, but periods of nineteen, eighteen, and seventeen days are frequent. There is some evidence that the period tends to be shorter when the person infected has been exposed to infection continuously, or at least for several days at the commencement of the illness of the infector. The shortest period is probably fourteen days, though some writers have believed that it might be only eight days (Biedert, Demme). The longest period known is twenty-five days.

The period of observation mentioned by the Code of Rules for the Prevention of Infectious and Contagious Diseases in Schools is twenty-four days, but it probably would be safer to extend this to twenty-five days.

Mumps is very infectious at the earliest stage, and during the pro-

dromal symptoms, if they occur. The prodromal stage, before the parotids begin to swell, should be taken to last four days, although it is not often so long. The risk of infection diminishes progressively from the onset of parotitis, and, as a rule, has ceased in three weeks from the beginning, probably in most cases in a fortnight, if all swelling of the parotids and all other glands have subsided.

The Code of Rules for the Prevention of Contagious and Infectious Diseases in Schools * directs isolation for four weeks, when the patient may mix with others if all swelling have subsided.

It should be borne in mind: First, that mumps is infectious in the prodromal stage; and persons who have been in contact with the patient during this stage, which must be assumed to comprise the four days before the onset of the swelling, even though no illness have been complained of, must be regarded as possibly infected, and kept under observation. By isolating persons first seen ten days after exposure to infection, it may thus still be possible to limit the spread of the infection by such persons, even if they subsequently develop the disease themselves. Second, since the incubation period is most likely nineteen to twenty-one days, and is sometimes twenty-four or twenty-five days, it is well worth while to isolate a person who has been exposed to infection a fortnight or even three weeks earlier. Third, isolation of mumps is easy, since the infection does not appear to be readily carried far from the body of the infected individual. Confinement of the patient to his own room is commonly effective in preventing spread through a household, if other children are excluded from all communication.

*Conclusions.*

1. The usual Period of Incubation of Mumps is three weeks. The shortest period is probably fourteen days. The longest period known is twenty-five days.

2. The Period of Observation should be twenty-five days.

3. The Period of Isolation should be twenty-eight days; and, if all glandular swelling have subsided, and there is no tenderness of the breasts or other parts of the body, the patient may be released.

* Code of Rules for the Prevention of Infectious and Contagious Diseases in Schools, issued by the Medical Officers of Schools Association, London, 1886.

## SCARLET FEVER.

### *Incubation.*

This period is very short, probably never extending beyond a week, and rarely lasting so long. It may be only twenty-four hours; but, on an average, it varies from three to five days. In 1861 Trousseau declared that neither in measles nor in scarlet fever could "the duration of the latent period be rigorously determined in the present stage of our knowledge." While, still later, Obermeier gave it as his opinion that the incubation period of scarlatina was unknown.

Murchison * collected 75 cases of scarlet fever, having any bearing on the question, which had either come under his own observation or been communicated to him in the twenty years ending 1878. Of the total 75 cases, in not one did the incubation period exceed six days. In 73 cases it could not have exceeded five days. In 54 cases it could not have exceeded four days. In 20 cases it could not have exceeded three days. In 15 cases it could not have exceeded two days. In 3 cases it could not have exceeded twenty-four hours. It also appears that the longest period of incubation made out in any of the cases was four and one-half days, and that in only 2 of the cases was it certain that it was as long as four days.

Murchison's conclusions are as follows: —

1. The duration of the incubation stage may be only a few hours.
2. Probably in a large proportion of cases it does not exceed forty-eight hours.
3. It very rarely exceeds seven days.
4. Consequently, a person who has been exposed to scarlet fever, and does not sicken after a week's quarantine, may be pronounced safe.

Moore gives the following: —

"A gentleman, æt. 20, went from Dublin into the country November 2, three weeks after the disappearance of the rash of scarlet fever. On his arrival, the same day, he saw two sisters, both of whom took to bed with scarlet fever November 8; *i.e.*, in six days.— incubation = five days."

"A boy, æt. 8, sickened with scarlet fever on January 12. His

* Observations on the Period of Incubation of Scarlet Fever and of Some Other Diseases (Clinical Society's Transactions, vol. xi. p. 238, 1878).

sister took ill on the 17th, and five days later another sister sickened."

"Two girls, cousins, slept in the same bed on April 5 and 6. One of them complained of sore throat in the night of April 5, and showed the rash of scarlatina on the morning of the 7th. The other sickened on April 10; *i.e.*, on the fourth day."

"Of two sisters, one took ill on Dec. 27, 1871, and was at once isolated. The other remained under the same roof, but without seeing her sister until they met by accident on Feb. 4, 1872. She sickened with scarlet fever seven days later, on February 11.

Caiger (Allbutt's System of Medicine, 1897, vol. ii. p. 132) says, in the large proportion of cases, between a single definite exposure and the first appearance of the fibral symptoms, it has been either two, three, or four days. In some well-authenticated cases it has been less than forty-eight hours, but not much less. On the other hand, it is sometimes as long as five days, and, in rare instances, even six. If the first case in a family was properly isolated and the necessary measures for disinfection scrupulously carried out, the safety of the other members is practically assured, if no second case has arisen before the end of a week.

Robinson (American System of Practical Medicine, 1897, vol. i. p. 584) accepts the report of Williams for the Clinical Society of London (*Practitioner*, July, 1894), who tabulates the opinions as follows: —

| | *Usual time, days.* | *Minimum time, days* | *Maximum time, days.* |
|---|---|---|---|
| Committee of the Clinical Society, 1892 | 2 or 3 | 1 | 7 |
| Bristowe, 1887 . . . . . . . . . . | 6 to 8 | often less | occasionally longer. |
| Guinon, 1892 . . . . . . . . . . | 4 or 5 | | |
| Strümpel, 1887 . . . . . . . . . . | less than 4 | | 7 |

Hamilton (*British Medical Journal*, June 3, 1894), from an experience of a severe epidemic among troops, claims that the period of incubation of scarlet fever is three or four days. In some inoculation experiments the time of incubation has been seven days. Bokai (*Pester medicinisch-chirurgische Presse*, v. p. 990) reports 2 cases of tracheotomy in which scarlet fever developed sixteen hours after exposure. Soerensen (*Internationaler klinische Rundschau*, Nos. 6 and 7, 1889) states that in 10 cases operated upon by Paget, in which

scarlet fever developed, the period of incubation was one day in 2 cases, two days in 3, and three days in 3 cases. He also says that in 9 out of 12 cases of puerperal scarlet fever the period was three days. It would seem probable from these figures that, where infection occurs through a wound, the time of incubation is shortened.

Unger (Diagnostisches Lexikon, 1895) says the period of incubation may be less than twenty-four hours where it enters the body through a wound. In other cases it is two to four days or four to seven days, but seldom more.

Crandall (Cyclopædia of Children's Diseases, 1899, vol. v., Supplement, p. 237) says the extremes in authenticated cases varies from a few hours to fifteen days. In 84 per cent. of cases Holt found the period to be less than six days, and in 66 per cent. between two and four days.

Williams (Twentieth Century Practice of Medicine, vol. xiii. p. 376) says scarlet fever usually comes on during the second or third day after exposure.

Reimer believed that in more than two-thirds of the 3,624 cases which he collected the disease came on within the first three days after exposure.

The Clinical Society's Committee collected 28 cases of disease following exposure for the usual time to a known source of infection. In 16 the earliest symptoms were observed before the end of the third day; in 10, before the end of the second day. In 2 the interval was seven days, and on 1 eight days. Including other cases observed with precision, there were 106 cases in which 52, or nearly one-half, began before the end of the third day after exposure; while 47 began on the second or third day. Eight days must be taken to be the extreme limit of the period of incubation.

The period of observation given in the Code of Rules for the Prevention of Infectious and Contagious Diseases in Schools is fourteen days; but ten days would probably be sufficient if care were taken to make certain that the person was free from all fever and sore throat, and if the fomites which have been in contact with the infection be disinfected.

Scarlet fever is infectious from the appearance of the earliest symptoms, and until desquamation has ceased, and all signs of

inflammation of the mucous membrane shall have passed away. Infection may be conveyed by the suppurating discharge of middle ear disease until a late period. The urine in consecutive albuminuria is also said to be infectious. The period of isolation should not be less than six weeks; and the patient should receive a series of baths, and should not be liberated unless "desquamation have completely ceased, and there be no appearance of sore throat" (Code). Under exceptional circumstances, however, the individual may be actively infectious as long as eight weeks after the commencement of an attack; and it is not safe to lay down any absolute rules on which laymen can be allowed to act on their own responsibility.

### *Conclusions.*

1. Scarlet Fever has an Incubation Period of two to three days, as a rule; but it may be eight.

2. The Period of Observation should be ten days, provided there is absence of fever and sore throat and all fomites are disinfected.

3. The Period of Isolation should be seven weeks from the appearance of the eruption and desquamation should have ceased, the nose and throat should be healthy, all complications should be over, and thorough disinfection of house, patient, and belongings should have been done before the patient is released.

## WHOOPING-COUGH.

### *Incubation.*

During this period no symptoms of the disease present themselves. Statistics are at variance regarding its length, and the fact that the incubation takes place so insidiously makes the determination of the exact duration of the incubation difficult in most cases. It probably lasts from two to seven days, with an average of three to four days. (Griffith, American System of Medicine, 1897, vol. i. p. 715.)

Eustace Smith (Allbutt's System of Medicine, vol. ii. p. 239) says it probably varies in different individuals. In one very clear case, noted by Dr. Bristowe, the period was exactly a fortnight; but according to some writers it may be as short as four days. The infectiousness begins with the earliest symptoms. Indeed, it is in the

early, non-spasmodic stage that the child is most likely to be a source of danger to others; for, when the whoop appears, his power of communicating the disease begins to decline. Still, it is not at an end until a period of at least six weeks has elapsed from the beginning of the attack.

Williams (Twentieth Century Practice of Medicine, vol. xiii. p. 378) says the period of incubation is somewhat uncertain. The usual period between exposure and beginning of catarrhal symptoms is seven to ten days. The characteristic whoop appears seven days later, as a rule, so that the interval between exposure and whooping is usually about fourteen days.

The Code of Rules for the Prevention of Infectious and Contagious Diseases in Schools recommends the period of observation to be twenty-one days; but probably fifteen days would be sufficient if the individual were examined carefully at the expiration of that period, and found to be free from all trace of catarrh of the fauces and pharynx and of all signs of bronchitis.

The infectiousness is marked in the earliest stage, and before the characteristic whoop begins. It declines rapidly after the acute stage has passed. On the authority of Williams, Weill, who in 1894 expressed the opinion that whooping-cough was contagious only during the premonitory catarrhal stage, has since (*Lyon Médical*, May 9, 1897) put his opinion to the test. On various occasions he permitted nearly one hundred young children, who had not previously suffered from whooping-cough, to be associated in the same ward for twenty days or more with children suffering from the disease during the stage of whooping. In only one case was the disease contracted, and in this instance the patient from whom the infection was derived was in the very earliest period of the whooping stage. In three small epidemics he was able to satisfy himself that infection was contracted from children who had not yet begun to whoop. Weill concludes that infection ceases very soon after the characteristic whoop commences, and that therefore in a family it is not the patient who is already whooping, but his brothers and sisters who have not begun to whoop, that ought to be isolated.

The Code of Rules for Prevention of Infectious and Contagious Diseases in Schools says that the patient should be kept isolated from

susceptible persons for six weeks from the commencement of the whooping, and should then be allowed to mingle with others only if the whoop and all characteristic spasmodic cough have ceased. If all cough have completely passed away earlier, which is not often the case, this rule may be relaxed and the period of isolation shortened.

### *Conclusions.*

1. The duration of the Incubation Stage of Whooping-cough is four to ten days.

2. The Period of Observation should be twenty-one days,

3. The Period of Isolation should be six weeks from the commencement of the whooping, or when the spasmodic cough, or whooping, has ceased, and whenever cough has entirely left.

## MEASLES.

### *Incubation.*

Ten days, as a rule, elapse between the reception of the poison into the system and the manifestation of the earliest febrile and catarrhal symptoms, or fourteen days between the infection and the appearance of the rash. These are Panum's estimates, based on his observations in a newly infected community of the Faroe Islands in 1848.

Hebra successfully inoculated with the nasal mucus, and found the symptoms showed themselves in eight days, the difference in time representing the effect of the resistance offered to the invading virus by the skin and mucous membrane. The stage of incubation is shortened after inoculation.

Moore gives the following experiences of March 26, 1876:—

A married lady, æt. 21, fell ill of measles. Her parlor maid had sickened with the same disease ten days previously; *i.e.*, on March 17. At 3 P.M. on the 26th inst. Mrs. A. B. felt thirsty, chilly, and fatigued, and her appetite failed. The rash appeared on the face of the patient on the 30th (fifth day). The fastigium, 103.4 F., was reached at 1 P.M. of April 1 (seventh day), when the eyes were suffused. Defervescence was complete on April 3. In this case the duration of the latent period was almost certainly ten days.

In the spring of 1877 both measles and scarlatina were prevalent in Dublin, and it was difficult effectually to isolate the incoming cases in the epidemic wards of the general hospitals. In 2 instances, at the Meath Hospital, scarlatina patients unfortunately contracted measles while convalescing. In both cases the symptoms of measles were detected on the eleventh day after exposure; *i.e.*, on the eleventh day after admission to the hospital.

Owing to the fact that measles is infectious from the outset, and that the first case in a household may be readily mistaken for a common cold, the attempt to arrest its spread by adopting preventive measures, especially isolation, is practically futile. Hence the difficulty of arresting an outbreak of this disease in a family or community. Should an individual have been exposed to the infection of measles, at least sixteen days' quarantine will be necessary before he can be pronounced safe.

A patient who has passed through an attack of measles should not be declared free from infection until at least three and, preferably, four weeks have elapsed from the first symptoms. It will be remembered that the stage of desquamation generally lasts until the eighteenth day. (Moore.)

Williams (Allbutt's System of Medicine, 1897, vol. ii. p. 102) says the interval between exposure and the prodromal symptoms is usually ten days, but may be as short as five and perhaps four days. On the other hand, it may be as long as fourteen days, in which case the rash would not appear until the seventeenth or eighteenth or even nineteenth day after infection. Though the period of about fourteen days from exposure to the appearance of the rash is that met with in the vast majority of cases, it is important to recognize that it may be four or five days longer, since such exceptional cases must be taken into account in estimating the period for which a susceptible person who has been exposed to infection should be isolated from other persons who have not had the disease. An interval of a full fortnight must be allowed; and at the end of that time the person must be free from fever, catarrh, and photophobia before it can be said that he has escaped infection.

Griffiths (American System of Practical Medicine, vol. i. p. 627) says the majority of observations agree in fixing the incubation period at ten to twelve days,

Hochsinger (Diagnostisches Lexikon, 1894, p. 155) gives ten days as the period of incubation.

Crandall (Cyclopædia of the Diseases of Children, vol. v., Supplement, 1899, p. 245) says it may range from nine to twenty-one days. Among 144 cases Holt found it to be between eleven and fourteen days in 66 per cent. Crandall has seen repeatedly the initial symptoms appear twelve days after exposure. He thinks twelve days is the most common period of incubation. Except in complicated cases, in which the catarrhal symptoms are prolonged, the period of infection is not over twenty-eight days.

Williams (Twentieth Century Practice of Medicine, vol. xiii. p. 374, 1898) says the duration of the interval between exposure to the infection and the appearance of the rash is usually fourteen days. Almost as often it is a day more or a day less. The pre-eruptive, or prodromal, stage is of uncertain duration. It may be one day or may extend to five days. The incubation period of measles is, therefore, two or three days, as a rule, less than the interval mentioned above; and probably in most cases it is eleven or twelve days. It is believed to have a tendency to be short in cases where the subsequent attack is severe, with high and protracted pyrexia; and, on the other hand, a long period of incubation is said to be followed usually by a mild attack. When produced by inoculation, the period of incubation is said to be seven or eight days.

The period of observation recommended by the Report of the London Clinical Society is fourteen days, and at the end of that time the patient must be free from fever or catarrh.

The Code of Rules for the Prevention of Infectious and Contagious Diseases in Schools fixes sixteen days as the period of observation.

The infectiousness of measles exists from the earliest period of the developed disease. There is no notable diminution of the power of infectiousness during the whole acute stage, and there is no doubt that a patient may often be capable of conveying measles to another after convalescence has advanced far enough to allow him to go back to his usual avocations. It is well established that infection is more often spread during the prodromal period.

The period of isolation should extend to three weeks at least, and

the patient should be free from all desquamation and cough before being allowed to mix with susceptible children. The infective principle has a low power of maintaining itself outside of the body, and does not survive long in fomites under ordinary circumstances.

*Conclusions.*

1. The Incubation Period of Measles is eleven or twelve days. It may be ten or, possibly, shorter. On the other hand, it may be as long as fourteen days.

2. The Period of Observation should be sixteen days.

3. The Period of Isolation should be at least three weeks and sometimes four, and the patient should be free from desquamation and cough before being released.

## CHICKEN-POX.

*Incubation.*

Moore states: "Incubation. The stage of latency is believed to be, on an average, about as long as that of Small-pox; namely, twelve days. Makuna states that it varies from eight to seventeen days. Bristowe says that in some cases it lasts exactly a week, but perhaps more commonly a fortnight. According to Thomas, it varies from thirteen to seventeen days; while Trousseau extends the duration to from fifteen to twenty-seven days. On the other hand, Gregory limited it to from four to seven days. The discrepancy, in Dr. Hilton Fagge's opinion, arises from the fact that the length of the incubation has in general been calculated upon the very precarious basis of the interval between the dates at which different children of the same family have been successively attacked. Towards the close of this stage there is, according to Thomas, in some cases, a slight rise in temperature."

MacCombie (Allbutt's System of Medicine, 1897, vol. ii. p. 179) says that it varies from eleven to nineteen days, fourteen being most common. He has not seen it in less than thirteen or longer than seventeen. If inoculated, the incubation period is stated to be ten days.

Welch (American System of Practical Medicine, 1897, vol. i. p.

570) says, when the disease breaks out in a private family or an institution for children, the time that elapses between the appearance of the eruption in the first and second cases is usually from thirteen to seventeen days. This may therefore be regarded as about the usual period of incubation, although a much shorter period has been reported by some observers.

Unger (Diagnostisches Lexikon, 1895, p. 471) gives the stage of incubation from thirteen to nineteen days.

Williams (Twentieth Century Practice of Medicine, vol. xiii. p. 374, 1898) gives the interval between exposure to infection and the appearance of the eruption of varicella as usually fourteen days; and, since prodromal symptoms are very inconstant, this must be reckoned as the usual incubation period. It may be one day less, possibly, in exceptional cases, three days less. A period of nineteen days has frequently been established. The incubation period in inoculation is said to be ten days.

The period of observation, given by the Code of Rules for the Prevention of Infectious and Contagious Diseases in Schools is eighteen days.

Williams considers it safer to prolong it to twenty days.

During the period of incubation the patient, as a rule, presents no symptoms; but for a day or two before the eruption comes out there may be some *malaise*, and it is probable that infection may be contracted from the patient at this time. The patient continues to be infectious until convalescence is over and all scabs have become detached, especially of the scalp. Infection can probably be carried by fomites.

*Conclusions.*

1. The Period of Incubation of Chicken-pox is usually fourteen days. It may be from eleven to nineteen days.

2. The Period of Observation should be twenty days.

3. Infectiousness lasts until convalescence is over and all scabs, especially of the scalp, have been detached. This, then, should be the Period of Isolation.

## GERMAN MEASLES (ROETHELN).

### *Incubation.*

J. Lewis Smith, in the epidemic of 1874 in New York, did not find the stage of incubation to be uniform. In some instances it appeared to be from seven to ten days, and in other instances from eighteen to twenty-two days.

Thomas gives to this stage a duration of from two and a half to three weeks.

Moore observed in 1878 two cases, a brother and sister, the latter of whom sickened twelve days after her brother.

Williams (Allbutt's System of Medicine, vol. ii. p. 118, 1897) admits that the incubation period is not well determined, and says it is probably most often seventeen or eighteen days; but it may be two or three days more, or five or even, perhaps, seven days less. A person who has contracted the disease is capable of conveying the infection to others two or three days before the rash appears; that is to say, while he is himself quite free from any obvious symptoms of illness. The capability of infecting others remains during the presence of the rash, but declines rapidly, and in mild cases disappears in a week, though it may persist a little longer after more severe attacks or, perhaps, when there is much desquamation.

Griffiths (American System of Practical Medicine, vol. i. p. 640) gives as an average estimate, based upon the experience of many observers, a period of from one to three weeks.

Unger (Diagnostisches Lexikon, 1895) gives the incubation period as eighteen to twenty days.

Edwards (Cyclopædia of the Diseases of Children, vol. v., Supplement, 1899, p. 254) says the period is between seven and fourteen days.

Williams (Twentieth Century Practice of Medicine, vol. xiii. p. 375, 1898) says the incubation period is, as a rule, rather longer than measles. Prodromal symptoms are not always present or are so slight as to escape observation, and it is therefore necessary to reckon the incubation period to the date of the appearance of the rash. This is given by the Clinical Society's Report as eighteen days, but it may be more than two and less than three weeks with-

out being at all exceptional. It is occcasionally as short as five days. It seldom or never exceeds twenty-one days, although Baginsky (Lehrbuch der Kinderkrankheiten) admits twenty-four days.

The duration of the period of observation, according to the Code of Rules for the Prevention of Infectious and Contagious Diseases in Schools, should be sixteen days. The Clinical Society's Report is probably safer, which gives "two days more than three weeks."

German measles may be infectious for two or three days before the rash appears. It is in all cases infectious while the rash is out; but the infection declines rapidly, and in mild cases has probably ceased at the end of a week. In more severe cases it lasts longer, and risk cannot be considered as past so long as any desquamation continues.

The Code prescribes an isolation of two or three weeks, the exact time depending upon the nature of the attack.

### *Conclusions.*

1. The Incubation Period of German Measles is eighteen days usually, but it may be possibly five to twenty-one days.
2. The Period of Observation should be twenty-three days.
3. The Isolation Period should be fourteen to twenty-one days, according to the severity of the attack.

## SMALL-POX.

Moore says: "Incubation begins with the reception of the virus into the system, and ends at the appearance of the earliest symptom. Its average duration is twelve days, except in cases of inoculation, when it is only eight days, or still shorter, forty-eight hours, according to Curschmann. As a rule, there are no symptoms in this stage; but towards its close the patient probably feels unwell and out of sorts,—what the French aptly call *malaise*."

MacCombie (Allbutt's System of Medicine, 1897, vol. ii. p. 187) says it varies between extremes of five days and twenty or more. Usually it is twelve days, but not infrequently ten, eleven, thirteen, or sometimes nine. In a few exceptional cases symptoms of *malaise* occur from the date of reception of infection. Armstrong (*Lancet*,

1886, vol. i. p. 715) has given particulars of a case in which it appeared to be twenty-one days.

Welch (American System of Practical Medicine, 1897, vol. i. p. 522) gives it as from ten to twelve days. It is seldom less than eight or more than fourteen days.

Hebra (Diagnostisches Lexikon, 1898) gives the stage of incubation as between ten and fourteen days, seldom under that time.

Williams (Twentieth Century Practice of Medicine, 1898, vol. xiii. p. 373) says the initial symptoms come on, in a large majority of cases, on the eleventh or twelfth day, and the eruption appears on the fourteenth day. An interval of ten days only between the exposure and the initial symptoms is not very uncommon; and then the eruption appears on the thirteenth day. An interval of only eight days is rare; but Eichhorst has recorded three cases, one in a physician and two in medical students, in which the initial symptoms commenced in two of the individuals in nine days and eight hours after exposure, and, in the third, nine days and four hours. So short an interval is rare; but, when the disease is conveyed by inoculation, the initial symptoms appear on the eighth or ninth or, occasionally, on the seventh day. It is said the short periods are observed more often in hot countries than in temperate climates. There is some evidence that the incubation period of hæmorrhagic small-pox is rather shorter than that of the discrete form of the disease. Longer intervals are rather more common. Thus periods of thirteen, fourteen, and fifteen days between exposure and the initial symptoms are not very rare; and cases have occurred which appear to prove that it may be prolonged to twenty days.

The Report of the Clinical Society of London gives fifteen days as the *period of observation*, provided the individual at that time shows no signs of indisposition and presents no elevation of temperature. The Code of Rules for the Prevention of Infectious and Contagious Diseases in Schools gives eighteen days. Williams says it will be safer to put the period of observation at three weeks.

The infection can be preserved for a long period in clothes and other fomites, and in the hair of a person who has been in intimate contact with a small-pox patient. Cases are recorded in which the infection has thus been carried by nurses.

The disease is infectious from the onset of symptoms until all scabs have become detached and until all desquamation has ceased. Great care should be exercised in any case in which a suppurating discharge is left as a sequel to the disease.

### *Conclusions.*

1. The Stage of Incubation of Small-pox is eleven or twelve days usually. It may be eight days and perhaps twenty days.

2. The Period of Observation should be three weeks.

3. The patient may be released from Isolation when all scabs, especially of the hands and feet, have become detached, all desquamation has ceased, and no suppurating discharge left as a sequel, provided there has been thorough disinfection of everything.

## DIPHTHERIA.

### *Incubation.*

Osler (Practice of Medicine) gives the period of *incubation* as from two to seven days, generally two days.

Wood and Fitz say it is two days or more.

Hilton Fagge and Pye-Smith (Practice of Medicine) give as follows: —

Trousseau puts the period at from two to seven days.

Oertel puts the period at from two to five days, but he quotes cases in which it was longer.

Jenner relates a case of eight days.

Senator says that the interval may be as much as three or four weeks. Where there has been a direct transference of the poison from one person's fauces to another's, the period seems to be much shorter.

Valleix had a pellicular deposit on the tonsil the day after he became infected with the disease, and died in forty-eight hours.

Oertel supposes that the incubation period is likely to be less prolonged during the prevalence of an epidemic, especially where the type is malignant.

Williams (Twentieth Century Practice of Medicine, vol. xiii. p. 377) gives the usual period of incubation of diphtheria of the throat or

larynx as two days, and not often exceeding four days, although it never really exceeds this period, and is seldom less than two days.

The Code of Rules for the Prevention of Infectious and Contagious Diseases in Schools gives twelve days as the *period* of *observation*, which is ample, if the infection is spread by personal intercourse, and not by other means.

The infection of diphtheria may hang about a house in carpets, beds, etc., for months or perhaps years. A person may be infected by a patient in the incubation stage during the whole of the attack, and for a period of long but uncertain duration after apparent recovery. These conclusions, based on the study of the history of cases observed clinically, are fully confirmed by bacteriology, which has shown that the specific bacillus may persist in the throat for many weeks and even for months. As a rule, where infection has been thus transmitted at a late period, after the symptoms of the disease have passed away, some unhealthy condition of the tonsils or pharynx has persisted; and, all cases, in which such morbid states continue after diphtheria should be regarded as possible sources of infection to susceptible children brought into intimate relations with the patient. The infection may be contracted by intimate contact with the body of a person who had died of the disease.

The period of *isolation* after an attack of diphtheria which has satisfied the Medical Officers of Schools' Association is not less than three weeks after convalescence is completed, provided there is no longer any form of sore throat nor any kind of discharge from the throat, nose, eyes, ears or other parts, and no albuminuria. The termination should depend on the disappearance of all local lesions and the bacillus from the throat.

*Conclusions.*

1. The Period of Incubation of Diphtheria of the throat or larynx is usually two days. It does not often exceed four days, but occasionally reaches seven.

2. For a single exposure the Period of Observation should be twelve days.

3. The Period of Isolation after an attack of Diptheria should be not less than three weeks after convalescence, provided there is no evidence

of inflammation or discharge from the throat, nose, eyes, ears, or other parts of the body, and no albuminuria. Probably a better rule would be the obtaining of two consecutive negative cultures from the nose and from the throat before release of the patient.

## YELLOW FEVER.

### *Incubation.*

The period is comparatively short, probably never exceeds five days. Instances of attacks occurring inside of twenty-four hours after exposure are well authenticated. Several authors have insisted that the period of incubation may be prolonged two weeks or more; but this is probably an error, based upon the misinterpretation of the facts observed. (Sternberg, American System of Practical Medicine, 1897, vol. i. p. 281.)

Davidson (Allbutt's System of Medicine, 1897, vol. ii. p. 394) says the period of latency in yellow fever may be put down as ranging between twenty-four hours and four or five days. The length of time elapsing between the date of exposure and the declaration of the disease, which does not necessarily correspond with the period of incubation, has been known to extend to forty-five days or longer.

Weiss (Diagnostisches Lexikon, 1893) gives the period of incubation as two to three days, as a rule, with occasionally a longer or a shorter period.

Williams (Twentieth Century Practice of Medicine, 1898, vol. xiii. p. 383) gives the usual incubation period of yellow fever as four days, or a day more or less. Bemis (Pepper's System of Medicine, 1885, vol. i. p. 843) says the period may be as short as twenty-four hours, but states that in the three cases in which he was able to fix the hours of first exposure with precision the attacks followed in seventy-two, eighty-three, and one hundred and one hours respectively.

Williams thinks the period of *observation* of one exposed a single time ought to be a fortnight at least.

Williams says the infection is probably seldom derived from the patient direct, but it is easily contracted from fomites in which it may be preserved for long periods or from the structure or fittings of foul ships. He accepts the bacillus described by Sanarelli (*Annales*

*de l'Institut Pasteur*, June 25, 1895) as the infective agent, which is very resistant to drying, but is easily killed by direct sunlight. He says that under certain conditions, which would appear to be particularly liable to be produced on shipboard, it may be called into activity when otherwise unable to flourish by the growth, in its vicinity, of certain moulds,— a fact which may account for the manner in which the infection has clung to ships, springing into activity at irregular intervals.

The conclusions which we seem justified in drawing from the above are: —

1. The usual Period of Incubation of Yellow Fever is four days. It may be inside of twenty-four hours in exceptional cases or be extended to five days.

2. The Period of Observation of one exposed to the infection ought to be a fortnight at least.

3. The Period of Isolation should extend to the end of convalescence, and all articles in contact or near the patient should be disinfected and many of them burned.

## INFLUENZA.

### *Incubation.*

The period of incubation is brief, varying from a few hours to two or three days. This period is usually unattended by subjective symptoms. (J. C. Wilson, American System of Practical Medicine, 1897, vol. i. p. 407.)

Drozda (Diagnostisches Lexikon, 1893) gives the period of incubation from a few hours to one or two days.

Jennings (Cyclopædia of the Diseases of Children, 1899, vol. v. p. 312) says the period is from one to four or five days, and many cases develop within a few hours.

Williams (Twentieth Century Practice of Medicine, 1898, vol. xiii. p. 382) says the usual period is probably two or three days. The Clinical Society's Report concludes that it varies from one day or a few hours less to four or five days, but that the usual period is three or four days.

Parsons (Report on the Influenza Epidemic of 1889–90, Local

Government Board of England, 1891, pp. 63 *et seq.*) estimates it at two or three days. Periods as long as seven days have been reported; but during an epidemic of influenza it is exceedingly difficult to exclude the possibility of unrecognized exposure, owing to the number of ambulatory cases, and of mild attacks which the sufferers call common colds, and for which they do not undergo any period of isolation.

The period of *observation* after an exposure Williams concludes should be six or seven days.

"The patient is infectious throughout the acute attack, and may continue infectious until convalescence has advanced far enough to permit him to return to his ordinary avocations; and it is probably in this way that influenza is most generally spread. The period of isolation ought to be a week to ten days after the commencement of the disease, according to the severity of the attack. In cases complicated by pneumonia it should be extended to the end of convalescence." (Finkler, Twentieth Century Practice of Medicine, vol. xv. p. 51.)

*Incubation Period.*— In the German Collective Investigation Report, some cases are recorded in which the disease originated in a family in consequence of a definite importation; and in a number of instances physicians were able to determine in their own persons how many days after they had visited their first and only case of influenza they were themselves attacked. Based on these observations, the period of incubation was decided to be from two to six days in length. Very rarely it was as short as one or two days. The longest period that has been positively determined is seventeen days.

### *Conclusions.*

1. The Period of Incubation of Influenza is two or three days usually.

2. The Period of Observation after exposure should be six or seven days, according to the virulence of the epidemic.

3. The Period of Isolation of the sick should be a week to ten days after the commencement of the disease, according to the severity of the attack.

The President.—Fortunately, it is not necessary to insist upon the importance of such a paper as Dr. Cutler's to a collection of gentlemen charged with the important functions that the various boards of health of the Commonwealth are charged with: and I am sure they also appreciate the excellence of what Dr. Cutler has done for us, and I need not add how great will be the interest with which we shall look to the publication of it in full. Among all the powers that a board of health exercises, there certainly is no one that trenches so deeply upon the liberty of the individual as this right to quarantine; and there is no one that ought to be so safely guarded, there is certainly no one that ought to be so prudently and wisely exercised. I think that it is a matter that this Association cannot too much discuss or too much consider. The question is now open for discussion; and, if it helps any man to a conclusion, I think it is also open to judicious questionings.

Dr. Prescott.— Mr. Chairman and gentlemen, this is a subject, of course, in which we are all interested; and I am especially interested because I have charge of a school in which there are about one hundred inmates, and where we have a great deal of contagious disease of one sort and another, and it is important that we should know how long to keep a patient isolated after a contagious disease or how long to keep a patient under observation who has been exposed. During the last year we have had epidemics of diphtheria, scarlet fever, measles, and whooping-cough in the home. The diphtheria epidemic we were able to finally stamp out by taking cultures each day of every child in the home, and not letting any child who had the Klebs-Loeffler bacillus in his throat go among the other children. Dr. McCollom, at the South Department, was kind enough to take all the children who had Klebs-Loeffler bacillus in their throats, even without clinical symptoms, as long as he could, which helped us a good deal. It took about eight weeks to stamp that epidemic out. Of course it is the proper thing now not to let the child who has had a positive culture go among other children until after two or three negative cultures have been obtained. It was astonishing in how many cases there were positive cultures from throats that appeared perfectly normal. When the scarlet fever

started, we feared we were going to have as hard a time with that as we had with diphtheria; but we didn't, for it was not more than three weeks before there were no further cases of scarlet fever. It seems much more easy to stop an epidemic of scarlet fever than one of diphtheria. We had also an epidemic of measles, in which there were over forty cases; and with those we had a certain number of cases of cancrum oris. I don't know whether this disease has anything to do with the bacillus of measles; but it has so happened that in the last two epidemics of measles I have seen there have been several cases of cancrum oris and two or three deaths. We have been obliged to keep the patient under observation at least three weeks after the disappearance of the symptoms (in cases of measles) for fear of contagion for the other children. In whooping-cough the length of time we have kept the children apart was rather longer than Dr. Cutler has suggested, and it has been very difficult for me to make up my mind how long a child is dangerous to other children after an attack of whooping-cough. Owing to the danger of giving it to other children, I have been in the habit of insisting that the child with whooping-cough shall be isolated at least twelve weeks; and under these restrictions we have been able to stamp out an epidemic with less than a dozen cases among one hundred children. We were able to stop an epidemic of mumps with only six or eight cases by keeping them isolated three weeks. As regards the length of time necessary for isolation in cases of scarlet fever, I have never made up my mind definitely. Surely, until the end of the first desquamation, probably until the end of the second desquamation; but I do not believe that it is necessary, if there be a third desquamation, as sometimes occurs, to keep them isolated until that is ended.

Dr. W. H. Chapin (Springfield). — Mr. President and gentlemen, I wish to report a series of thirteen cases of typhoid fever which occurred in Springfield, Mass., in the present month. In June we had had very little typhoid. I only found 3 cases, and there appeared no common origin to those 3 cases. On the 15th of July a physician informed me that he had 2 cases of typhoid fever, who had gone to bed the day before. They were near neighbors. In the afternoon of the same day another physician reported a third case, at a distance from the first two. I found that evening that

these three people had a common milk supply; and on that information I sought the milk pedler and was able on the following morning to gather the facts which I will present to you to-day. The milk supplied to the three persons mentioned, and to the other ten which were afterward reported to me, was distributed from a milk station in Springfield in bottles, quart bottles. The milkman assured me — and I believe that he told the truth — that no milk was ever bottled excepting from three dairies in the town of Agawam; and to those three dairies — and, for certainty's sake, to a fourth dairy, which, however, he said had not been used in the bottling — I proceeded. In two out of the three there appeared to be no disease or uncleanliness about the place, and the same is true of the distributing station in the city; but upon the third farm I found that in the spring of 1899 a woman had been sick in bed three weeks, suffering from a disease which was diagnosticated to be grip, and she had some cough, and they said she had bronchitis. I don't know whether the diagnosis was right or wrong; but, at any rate, no other case of illness was found to have existed upon this farm. On the 29th of June, in the afternoon, the man who milked the cows upon that farm cleaned out the privy. His method was to half fill a wagon with sand, deposit the night-soil upon that sand, cover it with more sand, carry the whole to a waste place half a mile from the house, and bury it in the sand again. The wagon used for this purpose was an old, ordinarily unused wagon. After it had been used for this purpose, it was stored in a tobacco shed, at some distance from the other buildings, and had not been used for any other purpose since it was used to transport the night soil. I say in the afternoon of June 29, this man cleaned out this privy. He returned from that job, and did the milking. His product of milk for that night was forty quarts. He put all of those forty quarts of milk into a large mixing can, and filled his eight or ten quart can from that mixture.

This milk, transported to Springfield, and arriving at the milk station, was again mixed with an equal quantity of milk from the other two dairies, Nos. 1 and 2. So that there was bottled 120 bottles of milk, all of which was in part the milk of this suspected farm, and the rest from the unsuspected farms, so that, if on that day any milk was infected, 120 bottles were infected. Those bottles were

distributed to the man's customers and to the people who contracted the typhoid fever on the morning of the 30th of June, 1899; and I believe that on that morning these people, who were afterward sick, were infected with typhoid fever. Now for the succession of cases. As I say, in June we had had but 3 or 4 in town; and, besides the 13 cases which I am about to report, in July we have had but 2 others reported, one from the northern part of the town, at a great distance from these 13 cases, and the other in this same district. The one which does not appear to go in this class went to bed on the 15th of July, and took milk from another milk pedler. Remember, the milk was distributed upon the thirtieth day of June. On the seventh day of July a girl, not very strong, went to bed sick; on the 11th, four days later, another girl. Notice the sex and age of the sufferers. On the 13th of July, 2 women in one family and a boy in another family. On the 14th a woman went to bed, and afterward went to the hospital. She, living in this district on the 30th of June, left the town on the first day of July, went to Old Orchard beach, and returned on the fourteenth day of July sick, and went to bed. On the same day, the 14th, the first man in the epidemic went to bed; but he went to bed violently ill from the first. On the 16th, two days later, another man. He had been dragging around for a considerable time. On the 18th the husband of the Old Orchard woman went to bed, four days later than his wife. On the 19th, a young adult; on the 20th, another man; on the 21st, another man; and, lastly, on the 24th, the only exception to the series of cases, a woman went to bed. So far as I know, excepting 1 case that escaped us, left town, and was vaguely reported to be ill, this covers the epidemic.

Now in regard to the incubation of typhoid fever. The date of going to bed seems to me to be the only fixed point that we can take; and, if you will allow the bull, that is not a fixed point, either. But we don't know when symptoms begin in typhoid cases. The centre of the epidemic appears to be the thirteenth day of July, and that is fourteen days after the distribution of the milk.

If we allow that men in active life bear their troubles longer than women and children, struggle to keep about, and go to bed, finally, only when obliged to, we may assume that the men, all of whom fell

after the 13th of July, began to be sick as soon as the women, who fell just before the 13th of July; and, if we assume that they went three days longer than a woman would go, we can see that the list then would read: 2 cases on the 11th, 4 on the 13th, 2 on the 14th, 1 on the 15th, 16th, and 17th. The first case eight days after the distribution of the milk, the last case twenty-five days after the distribution of the milk. The morals that I would draw from this story are: first, that much can be learned by paying attention to a few cases; and, secondly, that the mixing of large quantities of milk in dairies or houses of distribution is a bad thing. It multiplies the possibility of infection remarkably. If I am right in assuming that this man infected his milk from his hands, it is probable that he may have infected but one pailful of milk, that only once did typhoid bacilli fall into his pail; but he proceeded immediately to infect every drop of milk of that milking, and, that not being enough, the milk pedler multiplied it by three. And instead of having 6 quarts of infected milk we had 120 quarts.

Dr. Durgin.— Gentlemen, it was intended by this paper to bring together data for reference, and upon which the different boards of health of our State might agree to act. One of the weak elements in our boards of health is differing with each other. One board of health will treat an infectious disease as dangerous for four weeks, another board of health will treat it as infectious for five weeks, another one for six weeks; and the people not only suffer, but question our right, our intelligence, and our discretion. If we will first study this question carefully, and then unite upon the length of time for the observation and isolation of these cases, we will strengthen ourselves immensely and please the public a great deal more. There is one thing to be said in regard to the great length of time occasionally noted for incubation and the time in which we should hold a patient as possibly infectious. Take quarantine, for instance. If you hold your vessel, the crew or passengers who are supposed to have been exposed, for the extreme limit of time, I believe you make a mistake. I will say, with perfect frankness, I do not believe that you can administer a quarantine or health office solely in the interest of public health. When we administer a quarantine or health office on extreme grounds, with both eyes closed to travel and commerce, we make a mistake.

I am aware that I am saying things which will be questioned, but I venture the suggestion that we may use judgment and reason even in treating public health questions. I think we should agree upon the length of time which would be reasonable to hold persons for observation who may have been exposed, and also upon the conditions under which we will release cases of infectious disease.

The President.— Dr. Shea.

Dr. Shea.— Mr. Chairman, our department for the past few days has been dealing with an epidemic of typhoid fever, and after investigation finds the source of the trouble an infected milk supply. In this epidemic our labors were of some result, and we were enabled to remove promptly a source of infection. Heretofore, in the great majority of the investigations that were made to trace the source of infection in this disease, our labors generally ceased when we reached the milk contractor, and for this reason. Tracing back any particular supply of milk, when we reached the contractor, he presented us with a list of two or three hundred farms, that we might examine as a possible source of infection these farms extending over New England. This, of course, ended our investigations for that particular supply of milk. However, these investigations were not barren entirely of results; for we had the satisfaction of knowing that the patients afflicted with this disease and the persons employed in the delivery of the milk that came under our jurisdiction were living and acting under good sanitary conditions, and this in itself amply repaid the labor and time devoted to this trouble.

Before speaking of this specific epidemic, I think it would be of interest to the members of this Association to acquaint them with the methods of the Boston Board of Health in dealing with this disease. When a case is reported by the attending physician, an inspector is detailed to investigate the sanitary condition of the premises where the disease exists. This consists in the examination of the drainage, plumbing, ventilation, whether or not well water is used, and, if so, an examination of that particular well, and especially the source of the milk supply. All these conditions are noted, and a written report made and filed at the office for reference.

About ten days ago our attention was called to this particular epidemic. As a result of our usual investigation, it was noticed that in

a certain section of the city 3 cases were reported in one day and the source of the milk supply was the same. These cases were investigated. Sanitary conditions of the patients' homes and surroundings were found in good condition. Next day 4 more cases reported; and up to the present time 26 cases have been investigated, and in all probability due to the same infection,— namely, the milk supply. All these patients obtain their milk from one dealer, and a visit was made to his plant. Here nothing was found but an ideal sanitary condition,— no sickness in the family or among the employees, a clean, wholesome stable, well drained and ventilated, supplied with city water, no well on the premises, milk-room also in good sanitary condition, floor clean, utensils for the care and handling of milk in a clean condition, such as we expect and demand from people in Boston that are engaged in the handling of milk.

Pursuing our investigations further, we were very fortunate to find that the supply of milk, which at that time was under strong suspicion of being the cause of infection, was obtained from a neighboring farm, about ten miles from Boston. This in itself was a source of satisfaction, as heretofore, as I have said above, our investigations generally ended when we reached the milk contractor. A visit was made to this farm the following day, and these conditions found: —

Previous history: Neighbors say that a boy died at this farm two years ago of typhoid fever. This statement denied by the father. Cause of boy's death, indeterminate. Last summer 2 boys sick with typhoid fever. Father says that they had slow fever.

Present history: Wife sick in bed one week. History: a sickness of four weeks' duration, diarrhœa three weeks ago, patient, up to the time of taking to bed, engaged in the preparation and care of milk, care of the cans and receptacles.

Examination of premises: On outside of house, large placard marked typhoid fever. House placarded one week before death of patient. Drainage from house on surface. Rear of house, barn, to this attached out-house, underneath barn cellar, afloat, two feet of water. Farmer acknowledges conditions to have existed for the last two years. Well about fifteen feet from house and barn. In the opinion of inmates, water from this particular well not fit for drinking purposes, only used for cooking and washing. The boy admits that

this water was used for cleaning the cans. Examination of water — much odor, dark color — confirmed the opinion of householders that it is not fit for drinking purposes, and also would add not fit for cleaning purposes, especially milk-cans. No sample, as, in our opinion, analysis unnecessary.

Gentlemen of this Association, I do not think that it requires any further evidence to convince you from the conditions I have described that in all probability these 28 cases of typhoid fever that we have at the present time owe their origin, in all probability, to this infected farm. Of course, the Health Department of Boston have no jurisdiction as regards the sanitary conditions on any farm outside the city; but we can, as we did in this particular case, forbid and prevent the bringing of milk to Boston from any infected farm.

Such, in brief, is a history of an epidemic that is the source of much anxiety to us at the present time. That the source of this infection was found and cut off so quickly is a measure of much satisfaction to our department, and, I am sure, also to a great number of innocent people that probably would have been infected without this summary action. The history of this case is one of great importance to this Association, and many lessons can and should be learned from its recital. Milk, in my opinion, should not be allowed to be sold from an infected farm, as was done in this case for a week after official notice was given to the proper authorities of the town. It is a duty that, as health officials, we owe to the public to prevent and forbid the sale of milk coming from an infected farm. It is only just to declare that milk from such a source should be declared unfit for use until it is certified by a competent health official that the danger of infection is past. Boards of health in the country can be of inestimable value as a protector and guard of a city's milk supply. The care of the water, examination at frequent intervals of the dairies, prompt investigation of any sickness and the cause thereof,— these and many more that I might enumerate would be of great importance. As a health officer, I find that these outbreaks of typhoid fever that we deal with in a large city like Boston, in the great majority of these cases, can be charged to infected milk. The milk supply of a city has been for a great number of years under strong suspicion, and justly so, in my opinion, for the greater number of cases of typhoid.

A city such as we have here in Boston, well sewered, with a water supply that is above suspicion, should not be dealing with epidemics of typhoid fever. It is for you, gentlemen, by your rules and regulations that you are able to make and have the authority to enforce, to declare that typhoid fever is a preventible disease, and as such should be stamped out.

Dr. Woodworth.— Mr. Chairman, I should like to ask Dr. Durgin a question. Should his views, just expressed, be generally adopted by the members of the Association, and provided that it was left to the judgment of each individual board to prescribe the allotted time that infectious diseases should be quarantined, how should we arrive at a uniform system by which to regulate the matter of quarantine? I understood that the one idea was for members of the State and various boards of the State to adopt a uniform system by which one board should not hold their infectious cases a much longer time than another; that, should any board hold their infectious cases or quarantine victims a time they deem sufficient to stamp out the disease, they should not be subjected to criticism or be told by the family or attending physician that there was no necessity of their holding them so long, that it was not necessary, that they need only have held them ten or fifteen days instead of three or four weeks, that neighboring town boards did not do it, and that the shorter time would do just as well. It seems to me that there are objections to the doctor's method of leaving this matter, and that, if we are to get good results or have a uniform system, we certainly cannot allow that each board may determine the length of time that the infectious cases should be held in quarantine. Perhaps the doctor can explain, so that we will understand it better.

Dr. Durgin.— If I did not make myself clear, I shall be glad to do so. My criticism is: first, that we have no standard; second, that some of us might use an extreme limit, which would be unjust in detaining convalescents from infectious diseases. It is a fact that there is no uniform standard adopted by the boards of health of this or any other State that I know of. There is a great variety of opinion as to how long we should hold patients or suspects. Now, then, if I were to make a suggestion as to getting a standard by which we should all work, it might be done through a committee of

the Association, who might consider the valuable data in Dr. Cutler's paper, and report at some future meeting what would be a reasonable time for us to agree upon for the official observation and detention periods for infectious diseases. I should be glad to see such a thing done.

THE PRESIDENT.—I hope Dr. Durgin will understand that he has made a motion, for it seems to me that it is the only possible way in which this matter can ever be settled; and I shall assume that he has made a motion. I should like to say, before putting the motion, however, that the laws of the State may possibly help us in a certain direction. The original statute which established the State Board of Health provided that that board should have, in case of epidemic disease, co-ordinate power with the various local boards of health of this Commonwealth. The State Board of Health has exercised that power but once in the whole course of its existence. The matter, however, has been considered at various times by the board; and, if this Association chooses to establish a committee, the State Board of Health would contribute with great willingness any of its officers to the consideration of that purpose, and I should offer up Dr. Abbott at once as a sacrifice. I know that he has considered the matter very thoroughly. If I can understand that Dr. Durgin has made a motion — in fact, I know he has — that a committee of five be appointed on behalf of this Association to consider this question, I will put that motion.

The motion was seconded.

THE PRESIDENT.—Is it your pleasure that a committee of five be appointed by this Association to consider this matter? If so, you will please signify it by saying ay: contrary-minded, no. (The motion was adopted.) How shall that committee be appointed?

VOICES.—Chair.

THE PRESIDENT.—If you will allow me, then, I will consider the matter and make up the committee later.

DR. MILLER.— Mr. Chairman, I was very much interested in the second paper, because the gentleman described a case so closely that I think I know the parties and think I saw the location yesterday.

It seems to me that in that case there ought to be some way by which something more can be done. He described 26 cases of typhoid fever, I understood him, traceable to the carelessness of one man, who keeps six cows; and the Boston Board of Health only had power to find out where that source came from. They have no power to make that man clean up his premises or to make a change; they simply have power to prohibit the milk from coming into Boston. But there is, so far, nothing to prevent him from selling that milk elsewhere; and, if he would leave his premises in that condition for two years, there is no probability, unless some greater pressure is brought to bear upon him, that he will clean up the premises now. It seems to me that there ought to be some law by which the State Board of Health, independent of the local board, can straighten out those cases; and I would like to ask for a committee similar to the committee that has just been voted — or the same committee, for instance, will do — to take this matter in to consideration, and see if in cases like that mentioned by the last paper we cannot have some law by which power can be brought to bear upon men of that stamp to compel them to clean up their premises. It is a fact known by a good many that the premises have been in the condition described for a long time. I don't think the writer of the paper has put it on near as thick as it will warrant. I passed by the premises yesterday. It seems that we ought to have some means of straightening up those cases. In that case there is no regular board of health, if I am not mistaken in the locality; but the Board of Health consists of the selectmen, who, as a rule, are so anxious for votes at the next March meeting that they don't like to disturb anybody. Compelling them to clean up the premises might lose a vote. I will recommend that that committee take that matter in consideration. I make that as a motion, Mr. President.

The President.— I put this motion very reluctantly, gentlemen, because it seems a reflection upon the intelligence of this Association. I should like to say, before putting that, that the power is absolute, of course, in the local boards of health. There is absolutely no power in the State Board to interfere in a matter of this sort; but there is absolute power in every local board of every town in this Commonwealth to stop just these things.

Dr. Miller.— Suppose your local board is dead?

The President.— Well, I don't think any one of us wants to believe that self-government has come to an end in Massachusetts. Dr. Durgin suggests that we ought to have a wake. You have heard the motion of Dr. Miller, that this committee which is to be appointed be empowered to consider the question of a further regulation of the dairies of the Commonwealth. Is it your pleasure that that matter be referred to the committee? Is there anything to be said upon that motion?

Dr. Durgin.— I recognize the position and the feelings of our friend who makes this motion; but there has been one long and patient effort made in this Association to cause all of the boards of health of this State to make, adopt, and enforce milk regulations. There was an obstacle encountered on the hill here in Boston; and none of the boards, except that of Boston, can legally utilize the milk inspectors. We were able in Boston to put those regulations in force. I believe, however, that every board in this State can make and enforce regulations enough concerning the milk supply to handle all such cases as those which were found in the town of Needham. I doubt about any town gaining aid from outside; that is, any material or legal aid. We are all ready to lend every moral aid that is possible, but I doubt about a town securing aid outside of its own borders. If it has a board of health which is asleep, there should be some means by which the town or its board may be woke up. I should be sorry to see such a subject referred to the committee which has been suggested, and I hope that the motion will not prevail.

Dr. Chapin (Springfield).— Mr. President, it has been my fortune to investigate milk farms at least a dozen times in the last four or five years. I have found no difficulty whatever in carrying the moral authority of the Springfield Board of Health even into the State of Connecticut, by the simple expedient of saying, "Well, we suspect this milk, and we really don't want to distribute it in our town; but, if you can do so and so, it will be all right." It has always been done so and so, and my authority is recognized in the towns of Suffield and Granby in Connecticut just as much as it is in Springfield. I don't think we need anything more than the moral weight of our Massachusetts laws.

Dr. Miller.— Mr. Chairman, it may seem a little outside for me to make that suggestion; but here are 26 cases of typhoid fever in the city of Boston, and the city has no remedy, you might say, simply it has now prohibited the milk from going into Boston. It seems to me too bad that the city of Boston should thus suffer. 26 cases, and we know not what more may follow, and yet have so slight a remedy. It seems to me that there ought to be some power by which the community could be protected more thoroughly than they are in a place of that kind. Many of you know that there are many small towns, as I suggested some time since, where the only board of health is the selectmen; and in the majority of cases they are not very efficient, so there seems to be a weak point, and it would seem as though we ought to have some power by which those cases can be regulated.

Dr. Woodworth.— Mr. Chairman, at Fitchburg we had something of the same experience some time ago; and, of certain farms that we inspected, but one man refused to comply with the request that was made. Inasmuch as these farms were in adjoining towns, and this one refused absolutely to do as was requested, he was prohibited from selling his milk in the town; and the milkman was informed that he could not bring that milk into the city, and he does not. We simply informed him that, unless he complied with the regulations of the board, we should not buy milk of him; and his milk has been refused, not allowed to be brought into the city. In all the others it was simply, as Dr. Chapin has said, that, if they would fix up, there would be no trouble, and they could bring up their milk as usual or sell it to the milkmen; and all but one complied. This one case of refusal is the only one we have had, and I think it is the only one in forty or fifty farms. This case occurred in the last seven.

A Member.— I suppose he went to making butter, didn't he?

Dr. Woodworth.— I cannot tell you what he is doing now.

Dr. Miller.— Mr. President, allow me one more say on this point. In this case that we are discussing, undoubtedly if the milk from these six cows had been sold into the town, something would have been done, the farm would have been investigated and things cleaned up; but, as that milk farm is pretty well one side and the milk went to Boston, nobody felt any particular interest — at least,

our local authorities felt no particular interest — in paying any attention to it. It was not sold in the town. Milk dealers there had nothing to do with it. The milk went to Boston. Consequently, it was none of their business. It was not their wash.

Mr. Pilsbury.— I should like to ask if there were any cases of typhoid fever in this town, and, if so, and they put up the cards, what they did about seeking for the source of contagion ?

Dr. Miller.— I think nothing yet.

Mr. Pilsbury.— It is time they did.

Mr. Jordan.— Isn't there a case of typhoid fever, or wasn't there one, right directly opposite this house, right on the opposite side of the street ?

Dr. Miller.— Yes.

Mr. Jordan.— Used this same milk ?

Dr. Miller.— I don't know about that.

Dr. Durgin. —It may be a bit of satisfaction to know that, when these cases are detected, and the milk from these farms is cut off, it has the most delightful quickening effect on the farmer that you ever saw. It has been so in these late instances. They get about their work, clean up the barn ; and they are very anxious for advice, and do whatever you tell them. In one of these instances the cows were removed at once from the farm. All of the cans and utensils used in the care of milk were thoroughly disinfected at once, and the place thoroughly cleaned up ; and, when this had been performed to the satisfaction of our own officers, then this milk will be readmitted into Boston. Meantime the farmer is losing money.

The President. — Is there anything else to be said upon this interesting subject ? If not, is there any other business to be brought before the Association at this time ?

Dr. Miller.— Mr. Chairman, I should like to ask a question. There were remarks made by the second speaker as to some of the diseases, so-called children's diseases. He spoke in the institution where he was of the effort they made in stamping out certain diseases. Take, for instance, measles and mumps for illustration. Suppose that in a town (not in a school), but in a town, isolated community, those diseases are mild, quite mild. Would it be advisable, in the opinion of the medical men, to make an effort to stamp them

out, or let them run the course, as they usually do? I thought of it when the gentleman was speaking. I merely asked, so as to get the opinion of the gentlemen present. I know it is quite common in many places not to make any effort, where those diseases are running mild, to stamp them out, but to let them run, and look after the children. In many cases they are so mild that the children are not kept indoors at all except in stormy weather, in other cases only a day or two.

The President.— Dr. Prescott.

Dr. Prescott.— I believe in trying to limit an epidemic of measles or mumps as much as possible, no matter how light the cases may be, because there is no knowing how soon in an epidemic where the cases have been light the cases may become severe and many patients die. I should say, Always limit the cases as much as possible, take just the same care with an epidemic of light cases as with an epidemic of severe ones.

Mr. Whitaker.— Mr. President, on this matter of milk supply, information has come to me of a milk inspector in a New England city who has a policy which is excellent, though it would not apply in Boston. He makes occasional inspections of out-of-town herds of cows supplying his city with milk. When he finds the conditions are bad, he reports it to the mayor, and the report is presented at the next meeting of the city government and ordered printed, and the publicity of that in the local papers remedies the evil in a short time.

The President.— Is there anything else to come before the Association at this time before we adjourn? Before that motion is put, however, I will name the committee for the consideration of the rules with regard to infectious diseases: Dr. Durgin, Dr. Abbott, Dr. Chapin, Dr. Darling, and Mr. Coffey, of Worcester.

Mr. Brimblecom.— Mr. President, I am sure we all appreciate the hospitality of the Boston Board which has been shown to us this afternoon; and I move that a vote of thanks be extended to them for their courtesy to the Association.

The motion was seconded.

The President.— You have heard the motion. It alludes to something which has become so much a matter of fact here that I am

afraid the Association does occasionally forget the very great debt that we are under to the city of Boston. It shows us all good things, in the first place, in a sanitary way; and it provides us with all good things for our bodily support and entertainment here. I will ask you, then, to assent to the vote by a rising vote.

The vote of thanks was adopted, all the members rising in its support. The Association then adjourned.

# JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH

## October Meeting, 1899

SUBJECTS: Rapid Diagnosis of Diphtheria by Direct Swab Examinations.—Food Infection by the Typhoid Bacillus

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# THE JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

The Massachusetts Association of Boards of Health was organized in Boston in March, 1890, with the following objects: the advancement of sanitary science in the Commonwealth of Massachusetts; the promotion of better organization and co-operation in the local Boards of Health; the uniform enforcement of sanitary laws and regulations; and the establishment of pleasant social relations among the members of the Association.

All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

The Official Journal of the Association is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them. No part of this matter is printed in any other periodical.

The Journal will present, from quarter to quarter, a fair and adequate picture of the progress of practical sanitary science as applied to the needs of a modern community. The various subjects which are reviewed in the quarterly meetings of the Association are treated by experts qualified to speak from daily experience in Public Health offices, who, as men of science are careful to be scientific and comprehensive, and who, as public officers, are no less careful to speak pertinently and so as to be easily intelligible to the layman.

The Journal, in a word, appeals to all whose interests touch the questions of sanitation and hygiene,— to the architect, the school-committee-man, the manufacturer, the contractor, and, above all, to the busy practitioner who has no time for any reading but what is brief and to the point.

The subscription price of the Journal is one dollar a year, payable in advance. Single numbers, twenty-five cents. It is on sale at the Old Corner Bookstore, and at Smith & McCance's Bookstore, 57 Bromfield Street, Boston.

All communications to the Association should be addressed to the Secretary, **EDWIN FARNHAM, M.D., City Hall, Cambridge, Mass.**

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[The Association as a body is not responsible for statements or opinions of any of its members.]

Vol. IX. January, 1900. No. 4.

## OCTOBER MEETING

OF THE

## Massachusetts Association of Boards of Health.

The quarterly meeting of the Massachusetts Association of Boards of Health was held at the Town Hall, Leominster, on the afternoon of Thursday, Oct. 26, 1899, the Association being the guests of the Selectmen of the town. Dr. Henry P. Walcott, the President, presided, and, when the members had gathered around the dining-table, opened the formal proceedings by saying,—

Gentlemen, I wish to present to you Chairman Cook, of Leominster, who desires to welcome us.

### ADDRESS OF WELCOME BY MR. CHARLES C. COOK, CHAIRMAN OF THE BOARD OF SELECTMEN.

*Mr. President and Gentlemen,*— It gives me great pleasure, on behalf of the citizens of Leominster, to extend to you a most hearty and cordial welcome in our midst to-day. Among the various boards and commissions of this Commonwealth, this Board is, no doubt, the most important; for by your protection our cities and towns are guarded against unsanitary conditions, and the public is guarded

against dangerous diseases. From time to time these meetings are held; and the public is interested and instructed, and in a measure helps to carry out your wise suggestions. Still, your duties are fraught with hard work, and often with unjust and severe criticism. If there is any man in this world that needs that quality in his general make-up commonly known as "sand," it is the man who is a member of a board of health [applause]. Gentlemen, we have such a board in Leominster. They are so thoroughly saturated with sand that grit is spread all over their smiling countenances, and we re-elect them every year [applause].

Now, gentlemen, we want you all to enjoy yourselves, to have a good time. Let dull care fly to the winds, and let this day be one of the most pleasant in the history of this Association. The latch-string hangs on the outside of all our doors, and it is your own fault if you don't pull it and come in. I won't delay this banquet by making a speech, but throw you in a bunch all the good things that an orator could say in two hours and a half, and close by welcoming you again and again to Leominster [great applause].

Dinner followed. At its close business was proceeded with, the President introducing it as follows: —

The Executive Committee desire to present to the Association the following names of candidates for admission to this body: —

| | |
|---|---|
| Dr. J. J. Goodwin | Clinton. |
| Allen G. Butterick, Esq. | Lancaster. |
| Dr. E. A. Darling | Cambridge. |
| Dr. Frank L. Morse | Boston. |
| Dr. Otis H. Marion | Brighton. |
| Dr. Henry S. Rowen | Brighton. |
| Dr. Francis E. Corey | Westboro. |
| C. S. Henry, Esq. | Westboro. |
| Dr. Asa R. Pattee | Falmouth. |
| Dr. F. F. M'Carthy | Marlboro. |

Is it your pleasure that these gentlemen become members of the Association?

The gentlemen named were elected members of the Association.

The President.— The Secretary of this body, Dr. Farnham, is unavoidably absent from this meeting.

Mr. Pilsbury (Boston).— Mr. Chairman, I nominate Mr. Coffey, of Worcester, to serve as Secretary *pro tem.*

The motion was adopted.

The President.— Mr. Coffey is duly elected, and will read the records of the last meeting of this Association.

The Secretary *pro tem.* read the records of the previous meeting.

The President.— You have heard the records read by the Secretary. Is there any amendment or any addition to be made thereto? If not, they will stand as the records of the last meeting of this Association. Is there any general business to come before the Association at this time?

Professor Sedgwick (Boston).— Mr. President, while all the members are present and while the excellent repast which we have enjoyed is, so to speak, still in our mouths, I think that the Association may well return thanks to the Selectmen of the town of Leominster, to the members of the Board of Health of that town, and to the citizens whom they represent, for the very excellent entertainment provided for this Association to-day. The hospitality of Leominster has been generous, and it has been most welcome. I assure you, sir [addressing the host], as a representative of your town, that this Association deeply appreciates the hospitality thus shown and the courtesy extended to us. I move, Mr. President, that the thanks of the Association be returned to the citizens of Leominster and their representatives [applause].

The President.— That is a vote which the President of this Association has great pleasure in putting. All those in favor of it will signify it by saying ay.

The order of your exercises will be slightly interrupted — temporarily interrupted — by the fact that I propose a certain displacement of the papers here. Dr. Hill finds it necessary to leave rather earlier than the rest of us, probably; and the first paper presented to the Association will be a paper on "Rapid Diagnosis in Diphtheria by Direct Swab Examinations," by Dr. H. W. Hill, of Boston. I introduce Dr. Hill to this Association.

## RAPID DIAGNOSIS OF DIPHTHERIA BY DIRECT SWAB EXAMINATIONS.

From the Laboratory of the Boston Board of Health.

BY HIBBERT WINSLOW HILL, M.D.

*Mr. President and Gentlemen*,— By the courtesy of Dr. Walcott, I have the privilege of reading this paper out of turn, having to get away early to leave for Minneapolis. I desire to thank Dr. Walcott for allowing this.

An apology for again appearing before you may this time carry with it the excuse that I wish to present a method, as practised by us, which has proved a considerable time-saver in the diagnosis of diphtheria. The method is by no means new; but we have of late carried it out systematically in the laboratory of the Boston Board of Health, and the results may be of interest to you.

It is, of course, a matter of common knowledge that the usual outfit for bacteriological diphtheria diagnosis consists of a tube containing serum and another tube containing a swab, with a card for particulars. The physician rubs the infected swab on the serum, and sends the serum tube to the laboratory for incubation.

In some places — in Providence, R.I., and the State of New Jersey, among others — the outfit consists of the swab and card only, omitting the serum tube. On the receipt of such an outfit at the laboratory, the swabs are rubbed over the surfaces of serum in tubes by some one connected with the laboratory; and these tubes are then incubated. This method presents the advantage that the outfit never depreciates with keeping, as does that outfit which contains the serum tube. It has the disadvantage that it requires a skilled and careful person to receive the swabs and prepare the cultures for incubation, whereas in the more common method all that is required from the person who receives the cultures is the placing of the culture boxes in the incubator properly.

Both outfits imply the incubation of the serum tube, usually over night. Both outfits meet, however, all the requirements for a "rapid diagnosis," since all that is necessary for the rapid method is the

examination of the material which remains adherent to the swab after the latter has been rubbed on the serum, or, if this is unsuccessful, an incubation of the serum tube for six hours, followed by the usual incubation for fifteen hours.

The process of swab examination is of the simplest kind. A drop of water is placed on a glass slide, and the cotton end of the swab is rolled on the slide in the water, considerable pressure being used in order to squeeze the material thoroughly out of the cotton. The preparation is dried, passed over the Bunsen flame once or twice, stained with Loeffler's methylene blue in the ordinary way, and placed under the microscope.

In comparing this with the ordinary method, using serum tubes after incubation, it will be seen that an examination can be made from the swab within five minutes after its receipt, whereas the ordinary incubation requires fourteen or fifteen hours as a minimum. Indeed, if a culture is taken in the morning of one day, the result is not usually available until the morning of the next, or twenty-four hours later.

A wait of only five minutes is such a decided gain over a wait of fifteen to twenty-four hours that the question naturally arises why this method is not at once adopted, to the exclusion of the serum tube and incubation method.

I do not think that this rapid method will supersede the established method for the following reasons:—

*First.* The recognition of diphtheria bacilli under the microscope in the "natural" state, direct from the throat, is more difficult than their recognition in the "cultivated" state, after incubation. I shall refer to the differences in morphology later on.

*Second.* The swab, in being rubbed on the serum to inoculate the serum tube, may have most of the bacilli removed from it to the serum, so that the subsequent rubbing on the glass slide yields a preparation with few or no organisms in it. If, to avoid this, the swab is rubbed on the glass before it is rubbed on the serum, the organisms may all be left on the glass, so that the serum culture is a failure. Nor does it answer this objection to say that it would not matter whether the serum culture is a failure or not, so long as the organisms are present on the slide, for the reason, already referred

to, that the organism in its "natural" state is sometimes very difficult to recognize, so difficult indeed that a diagnosis cannot always be arrived at from an otherwise successful swab preparation. This objection might, it is true, be met by using outfits containing a serum tube and *two* swabs instead of one, with instructions to use one of the swabs for inoculation of the serum and to return the other, after rubbing it also on the suspected membrane, to its own tube, for direct examination. Simpler still, the physician may be directed to use the swab from the ordinary single swab outfit, for inoculating the serum in the ordinary way, and then, before returning the swab to its own tube, again rubbing it over the suspected membrane in the throat.

Both of these methods introduce, however, other complications, but perhaps they are worth trying.

*Third.* The rapid method is only reliable when the bacilli are found. A negative diagnosis from the swab is of very little value. You will remember that, when a serum culture is used, a negative result is obtained in five to ten per cent. of the cases for diagnosis which subsequently turn out positive. The number of negative swabs from positive cases is much larger even than this.

The rapid method is, therefore, of real value only in cases for diagnosis, not in cases for release, and only in cases for diagnosis when the result is positive.

The bacilli are usually more difficult to recognize from a release case than from a diagnosis case, even on a serum culture; and the difficulty is exaggerated in swab examinations. This is, however, of little moment; for the same haste is not required for release as for diagnosis, so that the serum method remains, in every way, the most reliable and satisfactory method for release purposes.

Our rules are as follows: —

Swab examinations are made only on cases for diagnosis. A request for the examination must accompany the culture. The result, if positive, is final. If the result is negative, the case is considered doubtful, but probably negative. If the result is unsatisfactory, no diagnosis is given.

In all cases, positive, negative, or unsatisfactory, incubation of the serum tube is proceeded with, in the positive cases as a confirmatory measure, in the others as a necessity for satisfactory diagnosis.

If the swab and culture comes into the laboratory before noon, a five-hour incubation of the culture is given; that is, it is put into the incubator at once and examined at 5 P.M. A negative or unsatisfactory result from the swab examination, made immediately on receipt of the outfit, may thus receive early correction, since it is often possible to find the bacilli on the serum after five hours' incubation, although the swab examination failed to detect them. Finally, the usual over-night incubation is proceeded with, because the culture may sometimes be found positive next day, when both swab and five-hour incubation yielded negative results.

It may seem that this procedure is a complicated one, giving much extra work. As a matter of fact, however, a careful and exact routine once established, it becomes a very easy and simple matter. The refusal to examine release cultures by this method reduces the unprofitable work very much, and the saving of time in positive cases for diagnosis is so great that it is well worth the extra trouble.

For the benefit of the bacteriologist who has not yet attempted the rapid method, a few words are added relating to the morphology of the "natural" bacilli.

The "natural" bacilli are usually smaller and much more regular in outline than the "cultivated" ones. They stain as intensely and sometimes show similar granules and vacuoles (so called). The red or purplish-red granules, common in cultivated forms and described in a previous paper, are also sometimes present. After a five-hour incubation the bacilli take on a morphology intermediate between the swab forms and the "cultivated" forms found after the ordinary fifteen-hour incubation. In brief, the swab (or "natural") and the five-hour forms from a case for diagnosis resemble somewhat the "cultivated" forms from a case for release late in the course of convalescence. The recognition of one is about as difficult as that of the other; and, if the preparation comes from the nose, it is not wise to make a positive diagnosis from the swab, unless extremely typical forms are found. Nor would I advise any man, however familiar with the cultivated forms he may be, to attempt diagnosis from the "natural" swab forms until he has made a series of careful comparative studies, which may easily be done by examining every day for some

weeks the swabs accompanying his positive serum cultures, as well as the cultures themselves. The faculty for recognizing all sorts and kinds of diphtheria bacilli can be cultivated until it becomes an intuition; but experience and practice are essentials to success, and it is not surprising that rules as to morphology cannot be given much more definitely than are those outlined above. On the other hand, I am convinced that no board of health whose bacteriologist will thus prepare himself for the rapid method will ever regret its careful and conservative use on the lines here laid down. A few statistics from our own work are appended, to show the results obtained so far.

Any one of twenty-seven different possible combinations of results may be obtained from the same case; for the swab may be positive negative, or unsatisfactory, the succeeding five-hour incubation may be positive, negative, or unsatisfactory, and the final fifteen-hour incubation may be positive, negative, or unsatisfactory. Thus a swab may be unsatisfactory, the five-hour positive, and the fifteen-hour negative, or the swabs positive, the five-hour positive, and the fifteen-hour negative, or the swabs positive, the five-hour negative, and the fifteen-hour positive, and so on to twenty-seven combinations. Of course, the commoner combinations are all three positive or all three negative, or one or more unsatisfactory, the others being positive or negative. The point I want to make is that the bacteriologist must not be discouraged in the face of apparently contradictory results in following this method. He must be prepared for some such, and remember that the positive results, whether from swabs, five-hour, or fifteen-hour examinations, overrule all others. A negative result has comparatively little weight at any time, and no weight at all in the face of a positive result. Our results have been as follows, most of them being obtained during the last four months: —

Sixty-six cases were examined by both swab and fifteen-hour culture. Of the sixty-six cases, thirty-two proved positive on fifteen-hour culture. The swab examinations from these thirty-two showed twenty positives, seven negatives, and five unsatisfactories. In other words, we were able to make a diagnosis by this rapid method in about sixty-two per cent. of the positive cases from fifteen to twenty-four hours earlier than by the ordinary method.

It is rather striking that in three cases on which the fifteen-hour

cultures were negative the swab was positive, being confirmed by subsequent cultures in two cases. The third did not present any further cultures. In these three cases, then, a positive diagnosis was furnished thirty-six to forty-eight hours earlier than by the ordinary method.

It will be seen that negative swab examinations are unreliable; for, out of a total of thirty-four negative swab examinations, seven were in error, or over twenty per cent. Moreover, the proportion of unsatisfactory swabs is high, reaching fifteen per cent. of the total number made.

The five-hour incubation is somewhat more reliable. Out of a total of forty-seven cases examined by both five-hour and fifteen-hour incubations, twenty-three proved positive on fifteen-hour incubation. Twenty of these were positive after six hours, the other three being unsatisfactory. By the five-hour method, then, we were able to save from nine to eighteen hours on about eighty-six per cent. of these positive cases. In four cases on which the fifteen-hour incubation gave a negative result the five-hour culture was positive. Two of these cases were confirmed by subsequent cultures. No subsequent cultures were obtained on the third case, and only release cultures on the fourth. On each of these four cases from twenty-seven to thirty-nine hours were saved.

In brief, given one hundred persons having diphtheria bacilli in their throats examined by all three methods, the results might be expected to be somewhat as follows, following the figures obtained in our own work: —

(It is supposed that one swab is taken from each of the hundred persons having bacilli in their throats. The swab is inoculated on serum, and the swab examined at once. The serum is incubated for five hours and examined, then incubated for nine hours more and again examined.)

100 swab examinations, 62 positive, 38 negative or unsatisfactory. Examined at once.

100 5-hour incubations, 86 positive, 14 negative or unsatisfactory. Examined in five hours.

100 15-hour incubations, 92 or 93 positive, 7 or 8 negative or unsatisfactory. Examined in fifteen hours.

Our work has shown that positive swabs sometimes come from cases which afterward prove unsatisfactory on further incubation; also, that positive five-hour results sometimes come from cases which afterward prove unsatisfactory after fifteen hours' incubation. Hence there is good reason to believe that a positive result by some one of the three methods at least would be obtained in every case, of the one hundred given above, thus reducing the working-error of bacteriological diphtheria diagnosis practically to zero.

Dr. Chapin, of Providence, has insisted, and very urgently, on the necessity for two cultures for diagnosis in doubtful cases where the first culture is negative. By the swab and five-hour incubation method the number of cases requiring two cultures for diagnosis is reduced, with a considerable saving of time.

NOTE.— By analogy with "wild" and "cultivated" plants, swabs or "natural" forms of the bacilli should be designated as "wild," culture forms as "cultivated."

THE PRESIDENT.— Does any member of the Association desire to ask the doctor any questions upon the matters brought up by this paper? If not, we will now go on to the reading of the next paper, "Food Infection by the Typhoid Bacillus," by Dr. Morse, medical inspector of the State Board of Health. Dr. Morse [applause].

## FOOD INFECTION BY THE TYPHOID BACILLUS.

BY FRANK L. MORSE, M.D.,

MEDICAL INSPECTOR OF THE STATE BOARD OF HEALTH.

During the past six months, epidemics of typhoid fever have been investigated in eight different cities and towns with reference to the origin of the disease and a total of two hundred and fifty-six cases studied. From this group of cases, many important facts have been obtained; and, on account of many of the cases developing directly from some food infection by the typhoid bacilli, they are here presented to you in the hope that future cases of the disease may be thus prevented. The investigations in reference to typhoid fever have proved, beyond a doubt, that every case without exception always develops from some preceding one. It is not always easy to trace the origin of a single isolated case of typhoid fever, more on account of the absence of all of the facts in the case rather than from a lack of the specific infection; and so, necessarily, we rely upon the appearance of an epidemic to be studied under favorable circumstances, in order to offer an opinion. And even in the course of an epidemic there is usually some one or more cases in which the connection cannot be or is with difficulty established.

The bacteriological examination of the urine and fæces of most patients ill with typhoid fever show the presence of the specific organism which causes the disease, and to this fact is due the development of other cases. The organisms appear early in the course of the disease, even before the patient is ill enough to go to bed, and remain in the excretions even long after convalescence from the disease has been established. In this way the disease is often unconsciously spread, both before the sickness is at its height and after apparently complete recovery.

The most common mode of propagating the disease is through the medium of milk; and in the study of this group of cases one hundred and forty-seven patients were visited in which the milk supply seemed to be the cause of the disease, with the result of finding one

hundred and ten patients, or nearly seventy-five per cent., taking milk from the same milkman.

These epidemics occurred in five different cities and towns; and in every case some person connected with the milk route was found ill, three of the men having unquestionable cases of typhoid fever, one having an abdominal sickness, diagnosed as appendicitis, and the remaining one having what was called influenza. It is a singular fact that by the time the discovery of the specific cause of the disease is made it is on the decline; for, if you will review for a moment the history of such an epidemic, you will observe that the original case, be he the one who does the milking or the driver of the team, feels poorly for several days before giving up work. During these four days in which the early symptoms are developing, he usually has some diarrhœa. The discharges contain the typhoid bacilli, and become lodged upon his hands and clothing. If he washes his hands, — which, by the way, is not usually done, — that means of infection could perhaps be averted; but, once having become contaminated, it is a comparatively easy matter to carry the organisms to the milk, where they propagate and subsequently infect the persons who drink the milk. Allowing from ten days to two weeks to elapse before these patients become ill with the disease, the original case is by this time sick in bed; and, consequently, the source of the disease being stopped, no other cases develop.

If the infection is not carried directly by the hands or clothing to the milk, it may be carried, particularly at the farm, by means of the fæces to the well, which usually is near the privy, where either the milk is cooled or the cans are washed. If the water of the well is boiled, that will kill the organisms; but it has been observed that, after the cans have been scalded, they are rinsed out with cold well water, thus allowing the organisms to remain in the can. To illustrate how easily sewage can get into well water, it is only necessary to state that the analysis of a suspected well examined only a short time ago showed nearly twice as much pollution as is found in crude Natick sewage; but, in spite of this fact, the well was used daily for drinking and cooking, and no suspicions were attached to it. Another way by which the disease is also spread is by eating different articles of food which are served uncooked, such as oysters, clams, greens, and

various vegetables. These foods, when they cause the disease, usually become infected in the process of fertilizing the ground upon which they grow with sewage which contains the undisinfected stools of a typhoid fever patient. Such an epidemic, comprising fifty-three cases, has been recently investigated in which these cases occurred by the eating of celery. They could be studied under most favorable circumstances, for they appeared in an institution having two seperate and distinct classes of patients. The cases occurred, with but four exceptions, among one class, and not among the other. Both classes had the same milk, water, and other articles of food; but celery, being an extra article of diet, was supplied to one class, and not to the other. The four exceptions above mentioned were patients who became ill with the disease during the latter part of the epidemic,— in fact, all of them on the same day, and just about two weeks from the outbreak of the epidemic, probably receiving their infection directly from a nurse who had the disease. The method of their sewage disposal, together with the presence of an unrecognized case of typhoid fever, furnished the original means for the development of the disease. The sewage was conveyed by means of a large iron pipe a distance of several hundred yards to one of two filter beds, and deposited thereon, the liquid portion overflowing into a brook and the solid portion remaining upon the bed. About one year ago an imported case of typhoid fever appeared, which went unrecognized until near the end of the sickness. The stools, necessarily containing large numbers of typhoid bacilli, were consequently deposited on the filter bed, and not disinfected. Later in the winter another case appeared under the same conditions, thus furnishing ample evidence of the infection existing on the filter bed. Shortly afterward the sewage was turned on to the second filter bed, the first allowed to drain off, and the solid portion taken to fertilize the grass ground and the celery bed. On account of the method employed in raising celery,— by banking the stock with the earth,— it seemed to be a favorable medium for the transmission of the disease; and shortly after it was supplied to these persons the illness began to appear. As the number of cases increased and a cause was looked for, the celery was suspected, and orders were issued for no more of it to be used; and some which was already prepared for eating was locked up, in order to prevent anybody using it.

It is a significant fact, however, that one of the farm hands who had access to it, either not knowing the restrictions placed upon it or through ignorance of the danger from eating it, did eat some, and in eight days was taken ill with the disease, thus apparently adding to the evidence as to what food caused the epidemic. Since such restrictions have been in force, the cases have rapidly decreased in number.

Another class of cases are those developing insidiously, extending over a considerable period of time, with no common origin. Such an epidemic of fifty-six cases has recently been studied in a small manufacturing town, unequipped with a sewerage system. This epidemic, lasting over a period of three months, is on the decline at the present time, but has not yet come to an end. The patients attacked are of the poorer classes, who on account of their surroundings have improper medical treatment and little or no nursing. In some crowded houses, with poor accommodations, from three to eight cases have appeared, extending over a considerable length of time; and they unquestionably receive their infection one from another. Where such an epidemic as this arises, some competent person should be employed by the health authorities to instruct the people in regard to the nature of the disease and the means by which it is spread, paying special reference to the disinfection of the stools and to a system of cleanliness about the house and grounds.

The remedy to prevent the development of typhoid fever epidemics can be briefly summarized: —

1. Disinfection of the discharges of every patient ill with the disease.

2. Isolation of the patient to such a degree as to prevent all possible contamination of articles of food or drink by the excretions of the patient.

These suggestions, although well known, should be applied in the earliest course of the disease; for it is at that time that most of the danger arises. And when in the earlier stages of the disease, particularly on a milk farm, typhoid fever is suspected, all precautions should be taken to prevent the pollution of the milk.

The President.— You have heard Dr. Morse's paper, and the subject is now open for discussion by the Association. It is the good fortune of the Association to have with us to-day one who has given a more complete, a more rigidly scientific character to the investigations of typhoid fever than anybody in this Commonwealth, one to whom we owe much for the introduction of a thoroughly complete scientific method and a consistent regard to the biological side of the question; and I hope Professor Sedgwick will be willing to say a little to us upon this subject.

Professor Sedgwick (Boston).— *Mr. President and Fellow-members*,— I am sure that you must have enjoyed, as I certainly have, this admirable presentation of a most excellent piece of work. In fact, so modestly and so simply has Dr. Morse told his story that I fear there is a little danger that some of us may not appreciate the real significance and magnitude of his work. In some directions it is, perhaps, a confirmation of work which has been done in other countries; but, as regards the celery epidemic, it seems to me that a new and distinct step forward has been taken. It has long been recognized that articles of food like celery probably sometimes carry typhoid fever; but I do not remember to have read of any case — even upon sewage farms, where vegetables are raised and obviously watered with sewage — in which celery or other vegetables have been conclusively proven to be the vehicle of typhoid fever. I may be wrong in this; and, if so, Dr. Morse will correct me. But this is certainly a most interesting discovery, and I like to feel that the good old Commonwealth of Massachusetts is keeping up its reputation in this kind of work. The Association will remember that Marlboro in 1894 furnished the first epidemic in America of typhoid fever due to infected skimmed milk; and, if I am right in supposing that this is the first epidemic conclusively traced to celery,— and it does seem to me very conclusively traced to it, for the reasoning seems sound and the proofs seem ample,—then, again, we have led the country, and perhaps the world, in actually proving a case of the conveyance of typhoid fever by this particular vegetable.

In regard to the remedies which Dr. Morse proposes, these, of course, are sound, but difficult, in the case of typhoid fever particu-

larly so,— so many unrecognized cases travel about, so many early cases do damage before they are discovered. While his remedies are all good and reasonable, there is, after all, only one final remedy, which is to beware of uncooked food under all circumstances, as far as one can. I don't mean to say that we should give up eating celery, I don't mean to say that we should stop drinking milk; but I do mean to say that, when possible, when we are uncertain as to the origin of the celery or the milk,— and especially when celery comes upon the table, as it sometimes does, in the whole-stalk form, so that, as you peel off the outer leaves and seek for the tender innermost "chit," or central portion, you see little black particles of dung down there, as I have often seen them,— it would be wise for you and me and everybody else to avoid that particular kind of celery. The custom has come in, and I see prevails here, of scraping the individual stalks. You, sir [addressing the host], have had the celery properly prepared for us to-day, as I observed, knowing that something of this kind was to come up [applause]. The celery has been scraped; and whatever typhoid germs may by accident have come upon the celery from some other town [laughter] have been removed, no doubt.

Celery is often eaten cooked, as we are all aware. And here again the great and the only final salvation will be; first, in avoiding dirt, or in the securing of absolute cleanliness; and, in the second place, in the use of cookery. The human race never made a discovery, probably, so important as the use of fire in cookery. Charles Lamb has pointed out to us the æsthetic and gastronomic importance of the discovery of cookery, but Charles Lamb was not a member of the Massachusetts Association of Boards of Health. If he had been, I have no doubt he would have dwelt also, and even more, upon the sanitary importance of cookery.

All this line of work, it seems to me, is admirable. It is by this painstaking seeking out of the causes of things, and then the eternal preaching of the remedy,— cleanliness, absolute cleanliness, perpetual cleanliness, and cookery by fire where cleanliness is not possible,— that we can alone escape all danger. Obviously, the time is not ripe altogether for these. We shall go on using uncooked milk. We shall go on using uncooked vegetables [taking a bunch of grapes

from the table]. These grapes from California! Who knows who picked this particular bunch of grapes in California? Very likely a typhoid patient [laughter]. And yet you and I will cheerfully eat those grapes. We have to run some risks in this life, but we shall be wise to run as few as possible; and we shall run the fewer, the longer men like Dr. Morse keep at work.

Dr. French (Clinton). — Mr. President, I think it would be of interest to the members present if these men who are studying the subject scientifically would give us their opinion as to the most thorough practical means for disinfecting the typhoid stools.

The President. — Dr. Morse, you seem to be called upon for an expression of opinion.

Dr. Morse (Boston).— There are certain regulations which are made by every local board of health just in reference to this question. I suppose from reading that the best method that is known is by the very liberal use of chloride of lime. That is a very powerful disinfectant. It is easily prepared, it is not very expensive, and, on the whole, it is thoroughly reliable. A strong solution in water should be added to every typhoid stool, including both the fæces and the urine, and allowed to stand in it for certainly half an hour,— longer, if possible. In that time it is supposed that all the typhoid fever bacilli will be killed.

Professor Sedgwick (Boston).— Mr. President, I meant to have referred in my previous remarks to the excellent recommendation of Dr. Morse that, in a village where there is no regular board of health or no adequate supervision, it is often possible to secure such by going to one of the selectmen, and telling him just how to do it. In the Bondsville epidemic, which I had the honor of investigating, in 1892, we had a case of that kind. It was one of the "secondary infection" epidemics, like the one reported by Dr. Morse; and there was genuine alarm in the town and a strong desire to do the right thing. I recommended to the selectman — who was, of course, a member of the Board of Health — that he should appoint a bright physi-

cian in that town, and make him responsible, make him for the time being a health officer, holding him responsible for the proper carrying out of the disinfection. Then I advised him to purchase a large amount of unslaked lime, and to distribute this free of charge; to see that everybody in whose house there was any typhoid or any threatened typhoid should have plenty of it; and to see *himself* that it was actually put in there, and that the physicians in charge did attend to its use. This was carried out very faithfully, I believe, with the result that the epidemic was choked quite soon,— as soon as could reasonably have been expected. The families affected were of the poorer class. They were not able to buy disinfectants, and did not know how to use them. Just one health officer, a temporary health officer going around, controlled the whole thing, and brought a force to bear which was very salutary. I think that that can be done in many cases where there is no well-organized board of health. The disinfectants at such a time should be provided by the town, free of charge, so that there should be no excuse for their non-use. As for the idea of instructing the people, I think that is good, too; but it is not quick enough. There must be a strong arm *at once* to insist on the proper use of disinfectants, and somebody to see that it is done. After that may well come instruction, and even with it may go instruction; but there must be first an authority, and an intelligent authority, with energy and executive ability and plenty of disinfectants. Then the rest will follow very quickly.

Dr. Burr (Boston).— Mr. President, in connection with Dr. Morse's paper it might be well to report a recent epidemic which occurred in Boston. October 4, in looking over the typhoid records, we found quite a number of cases occurring in a certain district. As is the custom in all cases, the milk supply was looked into; and it was found that in the great majority of those cases the milk supply was from one milkman. He was immediately visited, and it was found that the physicians in the district had sort of intimated to him that they believed the epidemic in that district was due to the milk supplied by him. Becoming somewhat aroused at it, he reported to his contractor,— that is, the contractor who supplies him with milk,— stating to him that the physicians had thought that the milk supplied

by him was infected. The contractor that afternoon looked over ten or more different dairies which supplied this milkman, and upon one of those dairies a case of typhoid fever was found.

We went that afternoon directly to that farm. The farm is an average farm. I think you might call it a fairly good farm. I found that the son of the farmer had died on September 16 with typhoid. The son worked in Boston, and went home to the farm occasionally. It was his custom to go home on holidays. Saturday before Labor Day, September 1, I believe, he went home, as stated by his mother, because of feeling a little ill, and also to spend the holiday. Sunday, the following day, he was feeling ill, and Monday the same. Tuesday he went to bed, and on the 16th of the month he died. There can be no doubt but what the son was affected with typhoid in Boston; that is to say, this case, which afterward infected the milk at the farm, originated in Boston. In trying to find some possible source whereby the milk became contaminated while on the farm, I had quite a task before me. I will say that the case was very well taken care of, as told by the farmer's wife; but I found that the father was accustomed to sit with the patient in the sick-room, and among other things he did occasionally remove the stools to some portion of the farm. The stools were always treated with chloride of lime in the vessel, and also buried in the field, covered with lime again. It was stated that he removed the stools on a pitchfork. I don't just understand how he got them on the pitchfork [laughter]; but it must have required the use of the hands, at any rate, to get the vessel on to the fork, and also to throw them into the hole which he dug in the ground. I have no doubt he thought that he was taking excellent precautions. Even though handling the stools as above described, he also assisted in milking the cows. It was probably at the time of milking that the milk became contaminated from the hands of the father.

In referring to the epidemic, we find that from September 29 to October 13 ninety-three cases of typhoid were reported in Boston. Fifty-seven cases occurred in Dorchester, which is the district where this epidemic occurred. Forty-eight of those fifty-seven cases received milk from the same milkman. This milkman was a pedler of about fifty cans. October 13 we began to have a dropping off in the

cases. When you consider that the milk supply of Boston is about twenty-eight thousand cans daily, and this pedler supplied only fifty of those twenty-eight thousand, and that more than half of the cases in Boston, forty-eight out of ninety-three, came from this milkman's supply, you can see what a great epidemic may be caused by a very small percentage of the total milk supply of any large city. The case at the farm having originally come from Boston, the sanitary condition of the farm, I think, could be thrown out immediately, as playing any part in the source of contamination. But, to show you that the epidemic that we had must have originated from the milk supply of this farm, I will just give you the records of the cases which occurred in Boston previous to this epidemic. He went home September 1. Of course, his infection must have been some two weeks previous to that. From August 16 to September 1, the two weeks previous to the time he went home, only one case of typhoid occurred in Dorchester. For the entire month of August only two cases occurred in Dorchester. Then he goes home to the farm; and September 1 to September 29, when we noticed the beginning of the epidemic, only eight cases occurred in Dorchester. After September 29 we began to have a large number; and, as I say, from September 29 to October 13, when they began to decrease, we had forty-eight cases out of fifty-seven cases in Dorchester, all received from the milk of this one milkman.

As soon as the milk pedler found out that there was typhoid at any of the dairies in his milk supply, he shut off of his own accord the entire supply, receiving his milk from an entirely different locality. His place was disinfected in the usual way. The cans and bottles, etc., were all treated with a two per cent. formalin solution for four days. They were washed as the milkman always washed his bottles, then after that treated with the formalin solution for four consecutive days. The reason of treating them for four consecutive days was that in large cities the milk pedlers seldom get their cans and bottles back within forty-eight or seventy-two hours, in some cases even longer. I know that in his case the disinfection was carried out for at least a week.

The President.— Does any other gentleman desire to speak upon this subject or to ask any questions of the previous speakers?

Dr. Burr (Boston).— Mr. Chairman, I might add a word, that I understand within two or three days the father, or the farmer whom we suspect infected the milk, has been ill — he is ill now — with slow fever in the Provinces.

Dr. Bigelow (Leominster).— Mr. President, I think I understood by Dr. Morse's remarks something in regard to the source of contagion through the clothing. I would like to ask him to explain how much this disease can be carried through the clothing.

Dr. Morse (Boston).— I do not mean to convey the idea that the disease is carried directly through the clothing; but, as a person coming down with typhoid fever has considerable diarrhœa, when he attends the calls of nature, it is practically impossible for him to escape infecting both his hands and his clothing. And, having infected either one, unless they are thoroughly washed or disinfected at that time, the organism stays where it is placed. The next time his hands came in contact with his clothing, even if he had disinfected his hands previously, they would be reinfected. The danger would be from direct infection from his clothing by his hands to the milk rather than directly from the clothing to the milk itself. It leaves the clothing in an infected condition.

Professor Sedgwick (Boston). — Mr. President, perhaps I can help Dr. Bigelow a little bit as to his question. It is well known that laundresses are very frequently infected by handling clothing from typhoid patients. There are a great many cases on record.

As long ago as 1873 Dr. William Budd, one of the great authorities on this subject, made it perfectly clear that washerwomen had received infection by handling clothes which were infected and so infected their own fingers, and then their own food, and thus themselves. I myself saw a case like this on one occasion,— a case which is unpublished, because it was in a girls' school, and it was not considered desirable to have it made public.

A girl fell ill with typhoid fever, which she brought into the school, probably from Lawrence; for it was at the time when Lawrence was drinking the Merrimack River unpurified. Her night-dress became soiled with excreta. It went to the laundry. The chambermaid, who gathered up the clothing from the different rooms and was known to have handled this, just two weeks afterward came down

with typhoid fever. The clothing went on to the laundry; and the washerwoman also, two weeks after, came down with it. A table waitress in the same school, who sat at table with the laundress and the chambermaid, came down with the typhoid fever. I found that they had actually sat at the same table, and I got this statement: that the waitress of the scholars' table, who was eating at the servants' table, had actually had handed to her pieces of bread cut by the chambermaid and handled by the washerwoman; and from this infected clothing there apparently came a whole crop of cases among servants and pupils, and the school had to be closed.

THE PRESIDENT. — If the Association will pardon me for adding a personal experience, I would say that I have had in my own experience, in a hospital with which I happen to be connected, an instance of the production of typhoid fever by this sort of infection. I have always talked to the nurses there, as of course every other physician has in every other hospital, about the danger of dirty hands; and I have always impressed upon them that what I meant was not the obviously dirty hands, but the infected hands. The matron, who was a bright woman of middle life, not inclined to receive instruction upon subjects upon which she supposed herself to be thoroughly instructed, as is not uncommon with ladies of middle life and unmarried, rather resented the supposition that anybody in the hospital would be so unclean as to contaminate her hands with the fæcal discharges of the patient, and not wash them. I let the lesson go. My instructions I did not recall, however. I did not apologize.

Things went on, and we had a good deal of typhoid fever in our hospital. One patient was moved into a private ward which is in the main building of the hospital, and occupied a room next to that occupied by the matron. The matron became extremely interested in the case. She was in the room repeatedly. After a few days — after a few weeks — she became sick herself with typhoid fever. I was self-restrained enough not to tell her in the beginning of it, "I told you so." But I waited till she got well, and then I said to her: "When did you get that thing? Where did you get it?" "Oh," she said, "I don't know where I got it." "Well," I said, "I

told you not to dirty your hands. The nurse who took care of So-and-so in the next room says that you came in there one day when the patient had had an involuntary discharge of the bowels, and that you removed the sheets." "Oh, yes," said she; "but I looked at my hands, and I saw that I had not got anything on them." She did get something on them, she got it in her mouth, and she got typhoid fever. That was the sequence.

We have a belated nomination for membership in the Association, of Dr. P. F. Gorham, bacteriologist of the Providence (R.I.) Board of Health. Is it your pleasure that he be admitted a member of this Association? If so, you will please to signify it by saying ay: contrary-minded, no; and he is so admitted.

The programme ostensibly places me upon the list of speakers. There are a certain number of gentlemen here who have descended certain descents and who have climbed certain ascents in the Metropolitan water basin, who don't care to hear anything more about the Metropolitan water basin at present, probably. They have had an object-lesson, they have had a laboratory demonstration of what the basin means; and they are so numerous in this body that it would be a pity to inflict upon them anything more with regard to it. Seeing so ample a representation from Clinton here, I am going to say that the real interest in that basin will come when we begin to construct the dam. We shall be thoroughly at work upon the dam in the course of two years; that is, we shall begin next spring. The dam itself, however, will show its head above the bed of the river the year after that. I presume then that the Clinton Board of Health would insist upon our coming to Clinton for a meeting, and that we would then go out and see the dam. It would be a short walk, not a troublesome one [applause]. I presume we should also get something in the way of refreshment, having in view the fact that such a magnificent example has been set to them [applause]. I understand that Clinton indorses that; and therefore it would be useless for me to say anything more upon that subject, particularly as the train leaves for Boston in about five minutes. Is there any other business to come before the Association at this time?

Dr. Woodworth (Fitchburg). — Mr. Chairman, in behalf of the Board of Health of Fitchburg and the Mayor and the City Government, I wish to tender an invitation to this Association to meet with us in Fitchburg in April, if they have not already decided upon a place of meeting for that date.

The President. —This kind invitation will be referred to the Executive Committee, and will be considered by them and reported upon at the January meeting.

Adjourned.

Members of the Association had devoted the morning to visiting the Metropolitan water basin under the escort and as the guests of members of the Leominster Board of Health. Special trolley cars were provided for the occasion.

# JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH

## January Meeting, 1900

SUBJECT: A Diphtheria Outbreak believed to be due to Infected Milk

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# THE JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

The Massachusetts Association of Boards of Health was organized in Boston in March, 1890, with the following objects: the advancement of sanitary science in the Commonwealth of Massachusetts; the promotion of better organization and co-operation in the local Boards of Health; the uniform enforcement of sanitary laws and regulations; and the establishment of pleasant social relations among the members of the Association.

All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

The Official Journal of the Association is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them. No part of this matter is printed in any other periodical.

The Journal will present, from quarter to quarter, a fair and adequate picture of the progress of practical sanitary science as applied to the needs of a modern community. The various subjects which are reviewed in the quarterly meetings of the Association are treated by experts qualified to speak from daily experience in Public Health offices, who, as men of science, are careful to be scientific and comprehensive, and who, as public officers, are no less careful to speak pertinently and so as to be easily intelligible to the layman.

The Journal, in a word, appeals to all whose interests touch the questions of sanitation and hygiene,— to the architect, the school-committee-man, the manufacturer, the contractor, and, above all, to the busy practitioner who has no time for any reading but what is brief and to the point.

The subscription price of the Journal is one dollar a year, payable in advance. Single numbers, twenty-five cents. It is on sale at the Old Corner Bookstore, and at Smith & McCance's Bookstore, 57 Bromfield Street, Boston.

All communications to the Association should be addressed to the Secretary, **EDWIN FARNHAM, M.D., City Hall, Cambridge, Mass.**

Subscriptions and all business communications should be sent directly to the publishers,

SMALL, MAYNARD & COMPANY,

**6 Beacon Street, Boston.**

JOURNAL OF THE

# MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

*ORGANIZED 1890.*

[The Association as a body is not responsible for statements or opinions of any of its members.]

VOL. X. April, 1900. NO. 1.


## JANUARY MEETING

OF THE

## Massachusetts Association of Boards of Health.

The annual meeting of the Massachusetts Association of Boards of Health was held at the Parker House, Boston, on the afternoon of Tuesday, Jan 30, 1900, the President, Henry P. Walcott, M.D., in the chair. After the dinner the proceedings were as follows: —

THE PRESIDENT.— Will the members of the Executive Committee come to this end of the room for the purpose of a meeting? I would also ask that any member of the Association having in his possession names of persons to be recommended for membership in the Association will kindly give them to his nearest neighbor who happens to be a member of the Executive Committee or send them to this end of the table. It is moved also that a committee of three be appointed to retire and bring in a list of names for the officers for the ensuing year. Is it your pleasure that that committee be appointed?

The motion was adopted.

THE PRESIDENT.— The chair will appoint as that committee Mr. Pilsbury, of Boston, Dr. Ripley, of Brockton, and Dr. Mason, of

Fitchburg. I have here a list of the retiring officers for the year for the benefit of the committee.

Dr. Durgin, Vice-President, in the chair.

THE CHAIRMAN.— The Association will now listen to the reading of the records by the Secretary.

Mr. James C. Coffey, Secretary *pro tem.*, read the records of the October meeting at Leominster.

THE CHAIRMAN.— Any corrections to be made in the records as read? If not, they will be allowed to stand. Awaiting the report of the Retiring Committee, we will suspend the election of officers, which is the next business on the card.

The Secretary will read the names of those who have been recommended for membership by the Executive Committee.

The Secretary read the following list of names: —

| | |
|---|---|
| J. H. STICKNEY, M.D. | Boston. |
| HOWARD P. ROGERS, D.V.M. | Allston. |
| THOMAS K. CUMMINGS | Milton. |
| CHARLES P. MORRILL, M.D. | North Andover. |
| GEORGE W. BATCHELDER | Worcester. |
| L. M. PALMER, M.D. | South Framingham. |
| GEORGE BUCHANAN | Woburn. |
| VIVIAN DANIEL, M.D. | Watertown. |
| EBEN C. NORTON, M.D. | Norwood. |
| CHARLES B. FOWLER | Salem. |
| G. ARTHUR BODWELL | Salem. |
| WILLIAM O. SAFFORD | Salem. |
| GEORGE O. WHITING | Lexington. |
| GILMAN OSGOOD, M.D. | Rockland. |

On motion of Mr. Coffey the above-named gentlemen were elected to membership.

THE CHAIRMAN.— The next business on the programme is the election of officers for the ensuing year. Is the committee ready to report? Pending the return of the absent committee, we will listen to the annual report of the Treasurer: —

## ANNUAL REPORT OF THE TREASURER.

### Receipts.

| | | |
|---|---|---|
| Balance from 1898 . . . . . . . . . . . . . . . . . | | $751.64 |
| Received from interest . . . . . . . . . . . . . . . | $21.70 | |
| Received from annual dues . . . . . . . . . . . . . | 226.50 | |
| Total receipts . . . . . . . . . . . . . . . . . | | 248.20 |
| Grand total . . . . . . . . . . . . . . . . . | | $999.84 |

### Expenses.

| | | |
|---|---|---|
| Postage and revenue stamps . . . . . . . . . . . . . | $28.84 | |
| Printing . . . . . . . . . . . . . . . . . . . . | 23.88 | |
| Cigars and dinner for guests . . . . . . . . . . . . | 21.50 | |
| Clerical assistance . . . . . . . . . . . . . . . . | 6.12 | |
| Total expenses . . . . . . . . . . . . . . . . . | | $80.34 |
| Balance to 1900 . . . . . . . . . . . . . . . . . | | $919.50 |

Of this balance $709.16 is drawing interest.

Respectfully submitted,

James B. Field, *Treasurer.*

Examined and approved as correctly cast and properly vouched for.

J. Arthur Gage, *Auditor.*

I have here a summary of our financial standing for ten years. We started in ten years ago with $3 dues, which we kept up for six years, then one year at $2, and now three years at $1.50. The first year we had only 83 members paying assessments. It held somewhere between 83 and 88 for about five years. Then the Association started to grow; and in succeeding years the number paying was 112, 114, 137, 146, and last year 151 paid assessments,— 83 in the beginning, 151 last year. Owing to the foresight and wisdom of our Publishing Committee, we have been enabled, during the last two years, to reduce our expenses materially. Although it might be possible for us now to get along with an annual due of $1 per member, yet for many reasons it seems wiser to recommend that the present rate of $1.50 be maintained for a few years longer.

On motion of Mr. Coffey the report of the Treasurer was accepted.

The Chairman.— Is there any miscellaneous business to come before the meeting? Has the Nominating Committee yet returned? While waiting for the committee, I should like to say in behalf of the Committee on Prize Essays that it has been found impossible for the committee to report to-day, and the circumstances demand that the committee should have more time. Perhaps in the course of two weeks, maybe less, we shall be able to agree, and then publish in the *Medical Journal;* and the successful paper will be printed in the next *Quarterly Journal* of the Association. In behalf of the committee I would ask for two weeks' further time.

On motion of Dr. Marion the Association granted the committee further time.

Formal proceedings were suspended for a few minutes while waiting for the report of the Nominating Committee.

The Chairman.—If the Association will come to order, we will proceed with the programme, and then suspend when the committee returns with the nominations. The next business is a paper on "A Diphtheria Outbreak believed to be due to Infected Milk," by Dr. H. Lincoln Chase, of Brookline.

# A DIPHTHERIA OUTBREAK BELIEVED TO BE DUE TO INFECTED MILK.

BY H. LINCOLN CHASE, M.D.,

AGENT OF THE BOARD OF HEALTH, BROOKLINE, MASS.

The comparative infrequency of epidemics of diphtheria due to infected milk, and the importance of their early detection, seem to justify some record of such an outbreak that recently occurred in Brookline, Mass. During the summer there had been reported occasional cases of diphtheria in the town, the record by months being as follows: June, 3; July, 3; and August, 10. This amount of diphtheria in a residential town of 19,000 inhabitants, and located on the borders of a great city never entirely free from that disease, is scarcely worthy of special note; but the Board of Health, following its usual policy of isolating at least the first cases of any dangerous contagious disease appearing in the more densely populated districts, and especially those in tenement houses, opened its diphtheria hospital early in the summer, and promptly removed to it such patients as it seemed necessary should be isolated. The additional precaution was also taken, when the public schools reopened early in September, of making daily medical inspections of the pupils at the larger schools and those nearest the two districts that had furnished most of the cases of diphtheria during the summer. A little later this inspection was extended to all the public schools in town except a very small one in the extreme southern end; and the offer and recommendation to daily inspect the 200 pupils of the parochial school was also accepted, one case of diphtheria having appeared there. Investigation showed that the cases of diphtheria from each of the two districts mentioned had nothing else in common than the sharing of the same crowded space in and about their homes. Undoubtedly there were a number of light, undetected cases among them. Their milk supply was examined into, but threw no light on the question.

The total number of cases of diphtheria in Brookline reported in

September was 22, and the causes of the epidemic — if such it could be termed — seemed only those usually found under similar circumstances. On October 9 a girl of eight years, the child of a Brookline milkman, was reported by Dr. Blanchard to have diphtheria; and the diagnosis was confirmed bacteriologically. The writer visited the family, gave a full dose of antitoxin to the sick child and smaller doses to the mother, the little brother, and the sister of eleven years, and sent the patient to the hospital. Noticing at the same visit that the older sister appeared ill, and her throat showing exudate or membrane on the tonsil, a bacteriological culture was made, and next morning found positive, whereupon she followed her sister to the hospital, and the infected rooms were thoroughly fumigated with formaldehyde. Cultures from the father, mother, and brother were negative. Being apprehensive of infection of the milk, the inspector of milk, Dr. F. H. Osgood, was notified, and with the writer visited the premises, which were found to be unsatisfactory in regard to cleanliness. Although neither the milk nor the cans ever were taken into the house, the boiling water for scalding out the cans was brought from the kitchen. Satisfactory arrangements were at once made for boiling the water under a shed in which the cans were scalded out. The two milkers slept in the attic of the milkman's house, and also ate in his house, but not with his family, except that occasionally one of the children would eat with them. The men were directed to both board and lodge away from the house until further investigation could be made, and they did so. On questioning the two milkers as to sore throat, one admitted that the previous Sunday he had felt slightly ill, and that his throat had felt a little sore. A culture was made from each with negative results. A second culture from the throat said to have been sore was, however, made at the suggestion of the bacteriologist of the Board of Health, Dr. Francis P. Denny; and this culture also was found to be negative. The milkman was forbidden to take any part whatever in the handling or delivering of the milk until he should receive permission. As a large part of the milk from this dairy was delivered in Boston, the Boston Board of Health was notified. No cases of diphtheria having been traced to this dairy at this time, and every one on the place remaining well, the two milkers were allowed,

after a week, to return to the attic to sleep and to the kitchen to eat, and the milkman to assist in scalding cans as previously. All went well as far as is known until the end of the month; and it was hoped that without any publicity and consequent serious damage to the milkman's business all necessary steps had been taken, and that every source of infection was removed. Subsequent events, however, proved this not to be the case.

On October 30 a Country Club employee, a carpenter, became ill with diphtheria, and went to the Board of Health Hospital. He was followed on the 31st by a second employee, a jockey. On November 3 a third employee, a waiter, became ill with diphtheria, and entered the hospital. On the 6th still another employee, a girl of twenty-three, serving as ladies' maid, was found to have diphtheria, and went to the hospital, the diagnoses in all cases being confirmed bacteriologically. It was then found that among the customers of this milkman was this club, which, though it had a small herd of its own, took daily for its thirty or more employees milk from two sources, one can a day, eight and one-half quarts, being from the dairy in question. The milk used by the members of the club and their guests was wholly from the club's own herd. The physician who attends at the club, Dr. Blanchard, notified the writer, and assisted in making cultures from the superintendent and the remaining employees of the club, twenty-nine in number. Of these only one, that from the club's cowman, was found positive. One employee, however, failed to appear with the others, and was discovered next day in bed with a very sore throat, which was found both clinically and bacteriologically diphtheria. He went with the cowman to join their four friends in the hospital. Formaldehyde disinfection by the Board of Health was thoroughly done in the infected rooms, and other precautions were taken; and, though other cases were looked for, none have as yet been discovered among either employees, members, or guests. About this time — namely, in the first week of November — Dr. Sabine found a case of diphtheria on Walnut Street in a servant-girl; and within a day or two after the two daughters of the family and the mother came down with the same symptoms, also a child on Tappan Street, a patient of Dr. Sabine. The diagnoses in all these patients, as in those from the Country

Club, were confirmed bacteriologically. On inquiry at their houses by the writer as to the milk supply, he found it came from the same unhappy dairy that the Country Club employees had patronized. November 10, four days after, the delivery of this milk was stopped in Brookline. A twelfth case occurred in this same milk route, and was reported by Dr. George H. Francis. The patient was a boy of eight years, living on Roberts Street, a nephew of the milkman. He went to the hospital dangerously ill, but finally recovered, as did all the other patients in the milkman's Brookline route.

Now we must go back a few days. Before the investigation at the Country Club was completed, the dairy was visited again by the writer, and some interesting information gathered. The two little girls were away at the hospital. The milkman, his wife, and little boy all appeared to be well, as did also his new milkers. One of the latter, however, was a new hand. Inquiring when and why the old hand had left, it transpired that, while he was boarding and lodging away from the house, he became alarmed about himself and very restless. Finally, he left without telling why or where he was going. Although the milkman and his two milkers seemed now perfectly well, and their throats appeared normal, it seemed best in view of all the circumstances to make tests of the throat of all three men, which was done. The culture from the milkman was negative; but those from both milkers were positive, showing the Klebs-Loeffler bacillus present in very large numbers. The men went to the hospital, the delivery of milk in Brookline from the dairy was entirely stopped, as previously stated, and the Board of Health of Boston, where most of the milk was sold, was again conferred with, and was informed of all the later facts in the case. To settle any doubt as to the virulence of the bacilli in the two milkers' throats, Dr. Denny inoculated two guinea pigs of about five hundred germs each from these cultures, with the result that both pigs died in less than thirty-six hours. Although the writer made no effort to discover how many cases developed among Boston families, some ninety of which were said to be in the Boston route, six Boston cases in three families, one case terminating fatally, came to his knowledge in the same period that the twelve Brookline cases were being reported. It should be stated in this connection that in the case of each of the

twelve Brookline patients no other source of infection, such as exposure in school to similar cases, was found, though looked for; also, that all twelve patients had regularly used milk or cream or both from the dairy in question. It should further be stated that the very few other cases of diphtheria reported during this period were nearly all accounted for by exposure other than by milk; also, that in no other milk route than this one were there more than one or two cases of diphtheria; while, of the seven or eight families in Brookline taking this milk, four of them had one or more of the members ill with diphtheria.

On November 25 the writer again visited the farm, hoping to be able to recommend to the Board of Health that the milkman should be allowed to resume business in Brookline. This time he found the milking was done mainly by the milkman alone, he having dispensed with about half his herd at the time his two milkers went to the hospital. He was occasionally assisted, however, by his brother, who previously had done nothing about his milk business, but had worked in other departments of the farm. The writer tested the throats of both men with the result that the milkman's throat was found, as previously, free from germs; but there were found a few Klebs-Loeffler bacilli in the culture from the brother. The permission for the delivery of the milk in Brookline was therefore postponed. December 12 Dr. Denny reported the second successive negative culture from the throat of the milkman's brother.

Reviewing the whole affair, several questions suggest themselves, among them these. At the time the milkman's two little girls went to the hospital, did the milker who owned up to a sore throat, but who failed twice to show a positive culture, actually have diphtheria? This would seem to be possible, but rather improbable. Did the milker who left shortly after the children were sent to the hospital have diphtheria in the incubation stage at the time his throat was tested with a negative result, and later, recognizing the usual symptoms, leave because fearful of being sent to the hospital or of giving the disease to his room-mate with a throat recently inflamed and susceptible? This explanation seems not only possible, but rather probable. Assuming the latter supposition to be correct, and that the man then gave diphtheria to his room-mate, the other milker, before

he took his sudden leave, it is very easy to account for the infection of the new hand and of the milk by both men. When we remember that milk is one of the best culture media known for the rapid multiplication of the bacilli of diphtheria, also that Pfluegger's investigations prove that persons with bacilli in their throats spray the air freely with these bacilli while talking even in an ordinary voice, it is easy to realize how great a source of danger to other people an undetected case of diphtheria, however light, is apt to be if its possessor is working daily over milk. The early detection of every such case is, therefore, a matter of great importance.

THE CHAIRMAN.— Dr. Chase's very interesting paper is now before the Association for discussion. Perhaps, inasmuch as this paper differs slightly from the succeeding paper, it would be better to discuss it now, before the other paper is read. I regret to say, on account of the allusion to cases in Boston, that two of the three physicians who had in charge the looking up of these cases are unavoidably absent this afternoon. Dr. Chapin, of Springfield.

DR. CHAPIN (Springfield).— I did not understand how many cases existed in Brookline at the time that were not due to the milk.

DR. CHASE.— Very few during the period in which these were reported. There were some eight or nine in the diphtheria hospital, and perhaps the same number in the town. Almost all, however, were corralled right there in the diphtheria hospital, as they occurred in the tenement districts.

THE CHAIRMAN.— Are there no other questions to be asked or observations to be made upon this paper?

DR. BURR.— Mr. Chairman, Dr. Chase has alluded to the Board of Health of Boston in this case. Perhaps it might be well to say a few words in regard to the Boston Board of Health's position. I think, perhaps, from Dr. Chase's remarks, that this Association might think that the Board of Health of Boston was notified twice. I don't know whether that is the fact or not, Dr. Chase; but, as I remember, the Board of Health of Boston received notice only on November 8, which was a month after the original cases occurred upon the farm. It might also be wondered by this Association why the Board of Health of Boston did not shut off that milkman's supply. It appeared

to us that, as long as the Board of Health of Brookline had removed the entire source,—that is, the two milkers at that time, November 8,—the possible source of infection or possible further infection of even the Boston customers had been removed. With that in view the milkman was called to the Board of Health of Boston, cultures were there taken, and he was instructed to have his cans and vessels used in milking and also in the delivery of milk thoroughly sterilized, washing in the usual way, and in addition to that disinfecting with formaldehyde, with which the Board of Health furnished him. It occurred to us that everything had been done that was possible to stop the further spread of diphtheria in Boston. It is rather interesting to note the very few cases which occurred in Boston when you consider the very large number of persons receiving that milk. There were some fifty or more families receiving milk from this particular milkman. Considering that the average number in each family is about five, then probably about two hundred and fifty people were receiving that milk, of whom only seven have shown any symptoms whatever.

Dr. Chase.—Mr. Chairman, in regard to allowing the milkman's business to go on, we did as Boston did, at the beginning. We reported, through Dr. Osgood, to the Boston Board when the trouble first began. We thought it was our duty to do it, knowing so much milk was sold in Boston. Then, in view of the precautions we took, the removal of those two children and isolating the men temporarily, we concluded it was best to let the business go right on; and we did so. Then later, in view of the later developments, we communicated again with the board, and were very willing to have those additional precautions followed out that Dr. Burr suggested. We did not stop the milk at the beginning. There had not been a single case reported on the route. Boston coincided in that view, and did not interfere with the business, either. After a month or more we thought it was our duty to stop it, and be sure. In view of the precautions taken the Boston authorities thought, on the whole, they could let it go on; and we don't know that there were any cases developed in Boston after the second notification. We thought, in view of the fact that we had perhaps erred a little the first time, that we certainly would not be caught that way a second time. But I think no new cases

occurred in Boston or Brookline later than four days from the time we stopped the delivery in Brookline.

THE CHAIRMAN.— It seems to me, gentlemen, that this offers a most interesting topic for every board in the State, and what occurred in Brookline is likely to occur in any other place within the Commonwealth. It seems to me that it is very creditable to Brookline that she should have so promptly and effectually investigated this case. If milk is, as I believe it may be, the medium through which diphtheria can be so easily communicated,— if that be the case, it becomes an interesting question for all of us. I thought the paper of Dr. Chase was one very worthy to come before the Association, and for every board of health in the State to know what can be and what ought to be done under like circumstances. I saw a moderate epidemic, in which perhaps eleven or twelve cases of diphtheria occurred in a remote part of Boston, some sixteen or eighteen, possibly twenty, years ago. We had not the laboratory at that time to aid us; but, so far as the investigation in a rough way would lead me, I concluded that the milk was the medium through which these cases had been communicated. I hope others of the Association feel the interest that I do in this milk question, and that there will still be more remarks concerning this interesting case.

DR. CHASE.— Mr. President, since reading my paper Mr. Stone has handed me this notice, stating that one of the Country Club employees, whom I never heard of, died at the Newton hospital of diphtheria. That would make the seventh case from the Country Club employees.

GEORGE WHITING.— I should like to inquire if Dr. Chase knows whether or not the same handler or handlers of the Brookline milk looked after the Boston supply as well, whether the milk was all mixed or kept apart.

DR. CHASE.— At the dairy they were the same.

MR. KEITH.— I should like to ask Dr. Chase if he considers that the testing of the throats of milkers would be a sufficient preventive measure for the protection of a milk supply, whether or not it would not be better, in other words, to shut off the milkman entirely rather than to allow the milk to be sold, as in this case, when the milk was drawn by parties not showing the Klebs-Loeffler bacilli in their throats.

Dr. Chase.— I think certainly the throat of everybody connected with it ought to be examined. We went through the whole family as well as the milkers. But, having found those men negative, we were sorry, in the light of subsequent events, that we did not shut it off at the outset. It seemed as if we had taken adequate precautions; and it is no light thing to shut off a man's business, unless you have more evidence than we had at that time. It would be wholly unprecedented, I am told by milk inspectors, if we had taken such action. It seems as if ordinary precautions had been used at that time. Any one can look back, of course.

Dr. Gage.— I should like to ask Dr. Chase whether if the milkman, when face to face with this problem, had said, "I will pasteurize my milk," he would have permitted him to have sold it or not, particularly as some milkmen deliver pasteurized milk.

Dr. Chase.— The milkman in this case was a very upright, honorable man. I have nothing but praise to say of his character. I think, if he had given his word of honor that he would pasteurize it, that I, personally, would have believed him; but I think that is rather an impractical question. I don't think any board of health would do that, and let the business go on in that way. I never heard of its being done when there was a question of contagion right on the farm. We did not think it was worth while to do that. His clients would not have taken his milk, I think.

Dr. Hill.— I should like to ask Dr. Chase if he has formulated in his own mind any theory of the exact way in which infection was transmitted, whether he thinks it came from the children originally to the milkmen, or through this nephew who came in finally, or what the direct course of the disease from one to another until it finally got to the milk really was. You may have stated this, doctor, in your paper; but I did not catch it exactly.

Dr. Chase.— I think that in some way some one of these milkers got it from the children,— it seemed so to me; then that the milkers in talking while milking and spitting about, as they do, and in salivating their hands, as milkers do at the beginning of milking, in some way introduced the germs into the milk a portion of the time.

Dr. Hill.— Your idea is, then, that the bacilli had been in the

throats of the helpers from the time the children were removed to the hospital in October — the 9th or 10th — until the end of the month, in spite of the negative cultures?

Dr. Chase.— It is a question in my mind. I can't say that I can answer that outright. But it seems as if, possibly, while the incubation stage was going on, those two successive negatives were not sufficient evidence of absence of disease from the milker's throat. It might have been overlooked that way. It seems as if it must have come some way from one of those two little girls to some one of those milkers, but I cannot piece it all out to my entire satisfaction.

Dr. Swarts.— Mr. Chairman, I think it would be interesting to know more about these seven cases which occurred in Boston, whether they came within a certain period, whether they might be connected with one can of milk or mess of milk, or whether the whole milking was the cause of those seven cases, or whether it was distributed through a number of different cans by the mixture. It is evident at the Country Club that the milk delivered there must have carried with it a considerable quantity of the infectious material from the farm; and, if there was no mixture there, it could be easily explained. As I understand, there was no mixture of the milk which went to the Country Club. If there was an individual can, of course it could be more readily understood. There is one thing, too, which I think might be considered in this connection; and that is the fact that a positive culture from the throat might have been followed, perhaps, by an examination of the nasal secretion, and a culture made there, as well as from the throat, as it is being found now in the various laboratories that examination of the nose will give a large percentage of positives, perhaps 50 per cent., and over, when it is also found in the throat, and it is frequently found in the nose when not in the throat. So, in such a case as that, where disease is present and liable to produce a disastrous result, it seems that examination of nose as well as throat for the whole members of the family would be desirable.

Dr. Chase.— In each examination I made, I made cultures from the nose as well as from the throat, making a man blow his nose upon a clean piece of cloth, then swabbing it from the piece of cloth. The milk that went to the Country Club was in an 8½-quart can.

Dr. Swarts.— A can by itself direct from the farm?

Dr. Chase.— Yes.

Dr. Swarts.— That was not true of the Boston cases?

Dr. Chase.— What went to Boston? I don't know about that. That went, I think, straight in his wagons to Boston. As soon as we began to supervise his work, he began to dispose of his cows. He put four or five of them in charge of another man in Brookline; and then he sold about half his herd, and cut it down in those two ways to quite a small number.

Dr. Miller.— Mr. Chairman, I should like to ask Dr. Chase if, in the cases where he examined the secretions from the nose and throat, he always found them the same,— if it was negative in one, it was in both; and, if it was positive in one, it was in both? This gentleman just made the remark that sometimes disease was found in the nasal secretions when it was not in the throat.

Dr. Chase.— In this case I put the mucus from the nose and the throat both into the same test-tube, putting that from the throat in the lower part, and reserving the upper half for the nasal. As a matter of fact, in my examinations I have often found the bacilli linger longer in the nose than in the throat.

Dr. Marion.— I should like to ask Dr. Chase if the herd itself was in healthy condition,— all the cows.

Dr. Chase.— Well, Dr. Osgood says none of them looked sick. There were a number of rather old cows which the man was willing to sell for beef at that time. They did not look sickly at all.

Dr. Hill.— It is rather hard to follow a paper read in that way so as to get the exact relation of each date. Have you definitely ruled out the possibility that the cases at the Country Club might have infected the milk-cans, the latter being then taken back to the original milkman and so infecting his supply? That suggestion has come up, and I should like to know what you have to say about it.

Dr. Chase.— I don't think that could have been. These men all came down at about the same time. All came down within about a week of each other, I think.

Dr. Hill.— But didn't the Country Club cases develop before the cases developed on the man's milk route?

Dr. Chase.— That club is a part of his route, you know. That is one of his large customers.

Dr. Hill.— Doesn't that leave the possibility that the case at the Country Club — the original one, say, at the Country Club — developed from some other source, and then the can was infected and went back to the milkman, and then infected his route, so that others got it from him?

Dr. Chase.— The contingency seems very remote.

Dr. Hill.— It has been suggested, and I simply wanted to know what you thought.

Dr. Chase.— I don't think people very often take any milk from a can and have it at table or round where these people could infect it, and then put it back into the can. I think they take a can and scald it out, or at least put it out. They don't keep the can round.

Dr. Hill.— But where were those who were handling the can? Didn't you get positive cultures in one of those men?

Dr. Chase.— No, the throat of the man that had the washing of the cans was negative every time,— that is, the proprietor; and these men at the Country Club, just as soon as they were found to have diphtheria, went at once to the hospital. They did not stay long at the club with sore throats. There was no place to keep them there. The club was very particular to get them removed at once. Even walking cases they sent right to the hospital.

Dr. Burr.— Mr. Chairman, I should like to ask Dr. Chase if he considered whether the help upon the milk farm and the Country Club as being connected in any way; that is, was there any communication between the help at the farm and the help at the club, which might account for these cases in some other way than through the milk supply of the farm. It seemed to me that the cases all came down pretty much at the same time; and, as I remember, a number of the Country Club cases among the help came down before the help upon the farm did. If I remember, the milker at the Country Club went to the hospital about the same time — and I think he was one of the later cases at the club — that the milkers from the milk farm did. I wondered if there was any communication between the help at both places.

Dr. Chase.— Yes, the milker there did go over about the same time as these others.

Dr. Hill.— Mr. Chairman and gentlemen, in this connection perhaps a very short review of the main cases on record might be of some interest, because Dr. Chase's epidemic is the first that I have been able to find any record of in which those directly associated with the milk,— in the handling of it,— the milkman and helpers, etc., showed positive cultures. There are 13 epidemics that I have been able to find since 1879 to date, spreading over, then, about twenty years,— 13 epidemics, that is to say, in which the milk was considered the source of infection. This included altogether 634 cases of diphtheria. The source of infection was not detected at all in 7, although efforts were made in that direction. In 2 there was clinical diphtheria in the family of those who supplied the milk. The milk dealer himself had clinical diphtheria in 2 cases. In 1 case the milk dealer had sore throat, which was not demonstrated to be diphtheria, although examinations were made some little time after he had recovered, about a week or so, and bacilli were not found. In 1 it was supposed that the discharges of a diphtheria patient had been thrown on to the bedding of the cows, so that the udders might have become infected in that way, and the milk infected while the milking process was going on. The cows in 4 cases were found healthy. In 3 cases they showed lesions on the udders, commonly known as chapped teats. In 5 cases the cows were not mentioned. In 1 they were healthy at the time, but developed chapped teats afterward. The main point about all this is that, as the milk question appears to stand now, Dr. Chase's epidemic is the only one in which bacilli have been found in the throats of those associated with the milk, so that, although these 634 cases of diphtheria, spreading over these 13 epidemics, have been traced to milk, it has been entirely by circumstantial evidence. Circumstantial evidence, however, carries considerable weight in the case of typhoid epidemics, in which the typhoid organism is practically never found bacteriologically; and circumstantial evidence ought to weigh in these epidemics as well as in Dr. Chase's. I think it seems established that milk epidemics of diphtheria may exist, even although the proof, as I think in Dr. Chase's own case, is not absolute,— that is, I mean the absolute bacteriological evidence. The circumstantial evidence certainly

points strongly in that direction, but it would seem as if no one of these milk epidemics had been absolutely proved other than by circumstantial evidence.

The Chairman.— Any further remarks to be made upon Dr. Chase's paper?

Dr. Denny.— Unfortunately, as Dr. Hill has said, we have no absolute proof that the infection took place through the milk. For that it would have been necessary to demonstrate the bacilli in the milk; but, unfortunately, we did not obtain a specimen which had been milked by the men who had the bacilli in their throats. The only other explanation for these cases could be that it was a coincidence; but it would certainly seem to be a very remarkable coincidence that there should have been twelve cases in Brookline among eight customers, and at the same time two milkers with almost pure cultures of virulent bacilli in their throats. One cannot help thinking that there was some connection between the milkers and those cases which developed.

Dr. Winchester.— Mr. Chairman, I should like to say, speaking of the cows, that I was able a few years ago, through the assistance of Dr. Ernst, to demonstrate what resemble very closely, if they are not identical with Klebs-Loeffler bacillus, the existence of the bacilli of diphtheria in cattle's throats; and Dr. Chase has the report.

The Chairman.— Any further remarks? Has the Committee on Nominations returned?

Mr. Pilsbury.— The Committee on Nominations submit the following list: —

*President.*

H. P. Walcott, M.D., of Cambridge.

*Vice-Presidents.*

S. H. Durgin, M.D., of Boston. Samuel W. Abbott, M.D., of Wakefield.

*Secretary.*

James C. Coffey, of Worcester.

*Treasurer.*

James B. Field, M.D., of Lowell.

*Executive Committee* (for two years).

G. L. Tobey, M.D., of Clinton. W. Y. Fox, M.D., of Taunton.
W. H. Gove, of Salem. Charles A. Hicks, M.D., of Fall River.
George H. Ellis, of Newton.

The Chairman.— Gentlemen, you have heard the report of your Nominating Committee. What is your pleasure?

Dr. Swarts.— I move that the report be accepted.

Mr. Pilsbury.— I move that one vote be cast representing the sentiments of the meeting, there being but one nominee for each office.

The Chairman.— It is moved and seconded that the Secretary of the Association be authorized to cast the ballot of the Association for this list of officers for the ensuing year.

The motion was adopted.

Mr. Coffey.— Mr. President, I cast this ballot for the Association.

The Chairman.— In behalf of the officers just elected, the best of whom are absent, and all being modest men, I wish to return the thanks of the officers to the Association for this compliment.

Mr. Coffey.— Mr. President, as my election as Secretary, for which I am thankful, has been accomplished, it will necessitate the resigning of my position as a member of the Executive Committee, with one year to serve; and I accordingly resign as a member of the Executive Committee, in order that it may be filled at this meeting.

Dr. Marion.— I move that the resignation be accepted.

The motion was adopted.

Dr. Mason.— Mr. Chairman, I nominate Dr. D. S. Woodworth, of Fitchburg, to take Mr. Coffey's place.

The Chairman.— It is moved and seconded that Dr. Woodworth, of Fitchburg, be made a member of the Executive Committee to fill the vacancy caused by the resignation of Mr. Coffey.

The motion was adopted.

The Chairman.— The next in order on the programme is a paper on "Bacteriological Investigation of Milk," by Simeon C. Keith, Jr., B.S., of Charlestown.

## ADDRESS OF MR. KEITH.

Milk as drawn from the healthy cow is practically free from bacteria; yet, unfortunately, it is not destined to remain so. The sources of the bacteria in normal milk are more numerous than one would naturally suppose at first. The so-called fore milk seems to always contain the germs of souring, and should be rejected if it is desirable to obtain a milk as nearly free from bacteria as is possible. However, more serious and greater sources of bacteria in milk arise from the introduction of hay dust, hairs, and loose cow-dung. With regard to this latter it is commonly thought that excreta can be removed from milk by straining it through cloth. But, as a matter of fact, this is not the case; for about one-half of cow-dung dissolves in the milk directly, and at the same time millions of fœcal bacteria find their way into it, and grow there, producing very unpleasant odors and tastes.

It has long been known that milk, as it is secreted in the cow's udder, is practically, if not entirely, sterile. Some ten years ago Professor Sedgwick and Mr. Batchelder succeeded in drawing milk free from bacteria by the use of sterilized milking-tubes. Last spring I tried a similar experiment in a somewhat different manner. I had an apron made so as to cover the entire sides and flanks of the cow, only allowing the teats to protrude. By this means all hair, dust, etc., was prevented from falling into the milk as it was drawn. The hands and teats were carefully washed with corrosive sublimate solution, and then about a dozen test-tubes that had previously been sterilized were successively filled from the same teat. Portions of these samples were at once used for making gelatine plate cultures, and the remaining milk was placed in an incubator at 98° Fahrenheit. These tubes were examined daily, and any change in the milk noted.

Tube I., containing the fore milk, coagulated at the end of two days ; and the corresponding plate culture showed it to contain 1,160 bacteria per cc.*

Tube II. remained sweet four days, and showed 180 bacteria per cc.

Tube III. Sample taken after a pint had been drawn remained sweet six days, and showed 20 bacteria per cc.

Tube IV., etc. Several successive tubes taken by sterilized milking-tube remained good for seven days, but showed no colonies or plates. Probably a few bacteria were present, but so few that they escaped detection.

It will thus be seen that, even without milking-tubes, it is possible to draw practically bacteria-free milk, if the dust, hair, etc., is kept out.

At the same time that the above milk samples were taken, I exposed a gelatine plate in the stable for two minutes, and found by the colonies developing on it that during that time several thousand bacteria had been deposited there with the settling dust of the stable.

On the average, milk sold in cities contains from seven to ten million bacteria per cubic centimetre, and we all know that the keeping quality and healthfulness of such milk is not all that it should be ; for, although, when kept cold, no apparent change takes place, yet, as Professor Sedgwick and Mr. Marshall have shown, there is a slow yet constant increase of bacteria that ultimately give rise to putrefaction, even though no souring change occurs. If the same milk, on the other hand, had been allowed to remain warm, even for a short time, the lactic acid bacteria would develop, and the milk would sour. So, although bacteria may be retarded in their growth by icing, yet, iced or not, either putrefactive bacteria or lactic acid bacteria are bound to develop.

Many of the aërator advertisements claim that aëration of milk will remove bacteria from it, and so prevent souring. But the real benefit derived from aëration comes from the fact that the milk, being charged with air, favors the growth of other than the souring bacteria, which thrive best when growing without air.

* About ¼ of a teaspoonful.

So that, while aëration, undoubtedly, prevents souring to a great extent, yet, on the other hand, it favors the growth of, perhaps, the less desirable putrefactive bacteria, and at the same time is likely to introduce large numbers of bacteria from the air as it comes in contact with the milk, especially if aërated in a stable or tie-up, where the average farmer is apt to do it.

With Messrs. H. P. Hood & Sons I have been enabled to visit a few dairies during the past year, and to attempt to trace the cause of the many high numbers of bacteria we found in the milk from them as received in Boston. With the dairies in question the following was the method of procedure. We first made bacterial analyses each week, until we knew what to expect from any given dairy that we were investigating. With this knowledge gained I visited the dairies, and sought to find out the cause of excessively high or low number of bacteria I had found, and in nearly every case was able to explain the reason for this.

Table 1 gives a general idea of the bacterial content in the milk found in ten dairies examined. The different numbers given show the number of bacteria per cc. in the milk, and in every instance examinations of the milk took place several successive days. It will be seen that dairies Nos. 1–7 and 12 showed very large numbers of bacteria; and a visit to the farm in each of these instances showed unsanitary conditions, and that the milk had received poor care.

Dairy No. 3 was interesting, not on account of the high numbers found, but for the fact that in each of the three examinations numerous bacteria were found in the milk, which gave evidence of its having been too near the pump. An examination of the well water on this farm proved the correctness of our supposition from the fact that it showed a pure culture of the bacteria we had found in the milk. The farmer was warned about rinsing his cans in this water, and seemed surprised that we were able to tell him his milk had been watered; and even more so, when we were able to tell him where the water in question was taken from.

Dairies Nos. 2, 4, 6, and 8, while showing comparatively large numbers as would appear to some, yet may be considered as model dairies in every respect; and from a bacterial point of view the milk was not seriously contaminated.

TABLE No. 1.

| | |
|---|---|
| **Dairy No. 1.** | |
| | 7,300,000 numerous liquefiers |
| | 225,000 |
| | 16,330,000 |
| | 10,000,000 |
| | 24,500,000 |
| **Dairy No. 2.** | |
| | 10,000 mostly B lactis |
| | 970,000 |
| | 30,000 |
| | 10,000 |
| | 10,000 |
| | 1,000,000 |
| | 10,000 |
| **Dairy No. 3.** | |
| | 3,410,000 green liquefiers |
| | 4,550,000 |
| | 3,600,000 |
| **Dairy No. 4.** | |
| | 15,000 |
| | 300,000 |
| | 970,000 |
| | 40,000 |
| | 65,000 |
| **Dairy No. 5.** | |
| | 135,000 |
| | 10,000 |
| | 60,000 |
| | 100,000 |
| | 50,000 |
| | 100,000 |
| | 600,000 |
| | 20,000 |
| | 250,000 |
| **Dairy No. 6.** | |
| | 15,000 |
| | 35,000 |
| | 900,000 |
| | 240,000 |
| | 30,000 |
| **Dairy No. 7.** | |
| | 15,750,000 |
| | 3,720,000 |
| | 4,000,000 |
| | 23,800,000 |
| | 9,500,000 |
| | 18,200,000 |
| **Dairy No. 8.** | |
| | 330,000 |
| | 330,000 |
| | 60,000 |
| | 20,000 |
| | 1,730,000 |
| | 90,000 |
| **Dairy No. 9.** | |
| | 12,440,000 |
| | 9,560,000 |
| | 21,700,000 |
| | 3,600,000 |
| | 6,860,000 |
| | 15,000,000 |
| **Dairy No. 10.** | |
| | 20,000 |
| | 135,000 |
| | 1,070,000 |
| | 400,000 |
| | 30,000 |
| | 50,000 |
| | 120,000 |
| | 1,100,000 |

It will also be seen, by referring to this table, that, on the whole, a bacterial analysis of a dairy reveals its character, and that two of

the same dairy on different days is an almost perfect indicator of what to expect of its condition.

In order to see what variation we should find in a number of cans of milk from the same dairy, in order to see what value could be given to our analysis, I analyzed nine cans of the same day's milk, with the following result, which seems to indicate, as did the analysis of milk of different days, that a very fair idea is to be had on our analysis. See Table No. 2.

**TABLE No. 2.**

| | |
|---|---|
| 55,000 | 220,000 |
| 420,000 | 50,000 |
| 750,000 | 100,000 |
| 270,000 | 725,000 |
| 700,000 | |

I have found, similar to Sedgwick and Marshall, that bacteria and acidity of milk have no fixed ratio. In other words, milk often shows high numbers of bacteria without of necessity showing acidity. On the other hand, however, as would be expected, I have never found acidity without large numbers of bacteria.

By means of peculiar flavors produced in milk by most lactic acid bacteria, expert tasters of milk are able to detect it before the acid is developed to any considerable extent. In other words, the taste of acidity is preceded by a peculiar flavor, which enables expert tasters to detect such milk before it is revealed by titration with standard alkali solutions. The minimum number of bacteria in milk of this character is about 20,000,000 per cc.

In many cases also I find that that milk thrown out by tasters does not contain high numbers of bacteria. This arises from the fact that often the cow eats something that renders the milk of bad flavor, or the milk has been allowed to stand in some place where it has absorbed foul odors, to which latter, milk has a great affinity.

I have since determined the numbers of bacteria in milk from several hundred dairies, and find, on the whole, that the bacteria present is rather lower than could be expected in view of the numbers present in most of the city milk.

In one lot of 67 dairies examined during the summer, 12 per

cent. showed less than 100,000 bacteria per cc., 50 per cent. 100,000 to 1,000,000 per cc., 25 per cent. 1,000,000 to 5,000,000 per cc., and 13 per cent. 5,000,000 and over per cc.

As I have said, however, there is some milk passed by the tasters that contains large numbers of bacteria; and it is my opinion, if dairy men did not of necessity have to mix their milk from different dairies to even up the amount of butter fat present, they would be much better off as regards the keeping quality and healthfulness of the milk, since a single can of poor milk will spoil a dozen cans of the best when mixed with them. Another source of the bacteria in city milk is the can in which it is delivered. Most milk-cans are washed with hot suds and rinsed in water. I have examined some 50 milk-cans to determine to what extent they may infect the milk, and find on the average there is a sufficient number remaining in a washed can to seriously affect the keeping quality of milk placed in it. The method of examining cans was to place 100 cc. sterilized water in a can, shake it up thoroughly, and empty back into the bottle. In this way, a part of the bacteria were removed with the water, and their numbers could be estimated by the usual process of plating.

Table 3 shows the numbers of bacteria found in 16 cans per cc. of water for rinsing.

**TABLE No. 3.***

| | |
|---|---|
| 68,000 | 800,000 |
| 560,000 | 568,000 |
| 13,000 | 406,000 |
| 728,000 | 301,000 |
| 798,000 | 377,000 |
| 2,240,000 | 665,000 |
| 110,000 | 26,000 |
| 180,000 | 332,000 |

This means that if a quart milk-can, like the above, were filled with sterilized milk, the milk would have at once from 2,600 to 224,000 bacteria added, which number alone would be large enough to cause it to change if the milk received improper care afterward, as is often the case.

* A large proportion of the bacteria found were of the lactic-acid-producing types.

### Summary.

1. Milk in its natural state is free from bacteria, and, consequently, will keep indefinitely.

2. Fore milk contains a few bacteria, and should be rejected.

3. Milk is seeded with bacteria from dust, contact with improperly cleaned utensils, water, etc.

4. Bacterial analyses of milk may be depended upon to indicate the care it has previously had.

5. A large part of the milk analyzed shows it to have had proper care.

6. Cooling milk does not entirely prevent growth of bacteria, but prevents souring.

7. The cans of the local dealers into which milk is put add a considerable number of bacteria to the milk.

8. That a bacterial examination of dairies is a valuable assistance in improving the condition of a milk supply.

The Chairman.— Gentlemen, Mr. Keith's valuable paper is before you for discussion.

Dr. Swarts.— I don't think, Mr. Chairman, that I fully understood about Table 1. As I understand it, those ten results, the results from the ten dairies, were milk taken under the conditions of a hood placed over the udder.

Mr. Keith.— The results were obtained from common milk drawn with usual care only, that is, just as the milk was received in Boston.

Dr. Swarts.— Then, the latter part of your paper, I was led to believe that you would have us infer that that contamination, such as there was, say, in dairies 1 and 3 there, came from the impure water in the cans. What I want to get at is whether it was the water which caused that or whether it was the detritus falling from the udder or the dirtiness of the hands of the attendants, or where that contamination came in dairies 1 and 3; and if in dairies 2 and 5, we will say, the sanitary conditions of the stable and the grooming of the animals, if any such grooming took place, were present there and absent in the other. What I should like to get at is what was

the cause of the contamination in 1 and 3, according to your opinion. And another thing I should like to ask, in the detail, the technique of it, is whether these individual numbers represent an individual plating or an average of a number of platings from one sample. Another question is as to what method you took to dilute your milk in order to get these counts,— that is to say, in one plate it would be difficult to count the two million in number,— what method or procedure you went under to get them down so that you could count them.

Mr. Keith.— These results in Table 1 were gotten from the milk as it was received in Boston from the dairies. In dairy No. 1, for instance, the five results might have been derived from milks received from that dairy at intervals of a day or so. In no instance, except in Table 2, has the milk been analyzed except on milks received from that same dairy on different days. The large number of bacteria found in dairy No. 1 was due, I think, entirely to the unsanitary conditions of the animals, stable, and care of the milk. The milk was not properly cooled, the animals were dirty, and, as we should say, the milk had not received proper care. In dairy No. 3, while the animals were perfectly clean, the infection evidently came entirely from that well water with which the cans were rinsed before putting the milk into them. The milk, of course, being warm, was especially favorable to the growth of these bacteria. Of course, the milk itself was a good culture medium into the bargain. So that the numbers in this case can be easily accounted for in this way. Dairies 2, 4, and 5 were model dairies in every respect,— clean men, clean animals, clean barns, and clean tie-ups.

Dr. Swarts.— What was the method of counts?

Mr. Keith.— The method that I employed for determining the number of bacteria is one that is ordinarily employed by bacteriologists,— a method of dilution, by diluting samples either one to one thousand or one to ten thousand with sterilized water and planting 1 cc. of the mixture, then multiplying my results by the amount of dilution. In other words, it is entirely impossible to count 24,000,000 bacteria in a single plate; but we can count 500; and, by diluting the milk sufficiently in a known ratio with sterile water, we can finally get them so low that we can count the colonies, when plated, and so find the number in the original milk.

DR. SWARTS.— In your dilution one to a thousand, was it done by shaking in a flask? and, if so, how long?

MR. KEITH.— It was done by shaking in a flask, by transferring 1 cc. of milk to 1,000 cc. of sterilized water, and then shaking thoroughly enough to get perfect mixture.

DR. SWARTS.— That would take how long?

MR. KEITH.— Only a few moments.

DR. SWARTS.— And, then, are these numbers the average of a number of plates or the individual plates?

MR. KEITH.— These are the individual plates. But, as I have said before, we have good reason to suppose that one examination of the milk gives a very fair idea of it, because on different days my numbers are not decidedly different with any one dairy in question. Of course, in some instances, they are different; but a farmer that is inclined to be dirty does not change his methods from one day to the other, and be clean one day and dirty the next. That is the point that I wished to bring out.

DR. GAGE.— Mr. Chairman, I have been listening to this paper with a great deal of interest, and thinking of the practical value it is to the milkman; but my mind goes back to the subject which I spoke of before this Association down the harbor some time ago, when the same subject was under discussion, and that is the fact that the doctor is chiefly interested in the pathogenic diphtheria that we get in the milk. I have recently seen some statistics from Europe, covering some cities in England and some cities, I think, in Norway and Sweden, where bacteriologists have reported that, in the samples of milk brought to the station by the cars to be delivered to that city, they have found from 15 to 24 per cent. of the samples to have the tubercle bacillus present. The veterinarians in my town or my city tell me that, so far as they can judge from their experience in the various farms or dairies about the city, anywhere from 25 to 50 per cent. of the cows are tuberculous. Dr. Ernst's experiments some years ago as to the percentage of tuberculous cows furnishing those bacilli in the milk, and these recent investigations as to the percentage in which the bacilli appear in milk delivered to large cities, it seems to me, make this question of exceeding importance; and I must say that, while I am

very anxious to get rid of the souring germ and the germs that cause putrefactive changes in milk, I am much more anxious to be sure that the tubercle bacillus can be kept out of the milk. I merely speak of this to bring it forward again. It seems to me it is a most fruitful source of investigation because of the dire results that come from drinking these pathogenic bacteria; and I should be very glad to know from the bacteriologists present if they can give us any new light as to the danger from the tubercle bacillus in milk, if any new investigations have been made locally, if it is not too far foreign to the subject.

The Chairman.— Dr. Theobald Smith present?

Dr. Smith had left the dining-room.

Dr. Swarts.— As to the question of the pathogenic bacteria, I think there is no question; but in the paper it is brought out that those bacteria, when they enter the intestine of the infant, become pathogenic bacteria, producing intestinal disturbance, and are of more importance to us, perhaps, than the tubercle bacillus, inasmuch as the number of children who die of diarrhœal diseases from dirty milk is a much greater proportion than those from tuberculosis, although I would not question the desirability of hunting for the tubercle bacillus if it could be easily found.

The Chairman.— Anything further to be said upon this paper?

Dr. Hill.— I might say, in reply to Dr. Gage, that I am very sorry that Dr. Smith is absent, because he has been working on that very problem. It seems to be a little uncertain at the present time just how much of tuberculosis in the human is really produced by bovine tuberculosis. Dr. Smith has had some articles on that subject in the last year or two. I am sorry to say that my memory has given out on the details; but, in general, it seems that the danger from tubercular cows is not as high as was formerly estimated. I think that is the general trend of Dr. Smith's work.

Dr. Chase.— Mr. President, one of the gentlemen mentioned to the meeting just now a report of Dr. Ernst on an examination of the throat of a cow that died with some mysterious symptoms; and I

will read this to the Association, with your permission. It is addressed to Dr. Winchester : —

HARVARD UNIVERSITY, MEDICAL SCHOOL.
BACTERIOLOGICAL LABORATORY, BOYLSTON AND EXETER STREETS,
BOSTON, March 29, 1894.

*Dear Doctor*,— We have found, in section of the material from the trachea of that cow, bacilli that in size and general appearance resemble very closely, if they are not identical with, the Klebs-Loeffler bacillus.

Dr. Whitney wants to know if you are going to be able to send that specimen for the museum.

Very sincerely,

HAROLD C. ERNST.

That, of course, is suggestive of another source of infection for that milk in Brookline; but I understand that the view now held by veterinarians is that cattle do not have the same form of diphtheria that human beings have. There is no bacillus in the disease resembling diphtheria found among calves, but a bacterium.

THE CHAIRMAN.— Is there anything further to be said upon this paper? I take this occasion to say that one of the papers submitted for the prize is not accompanied by the sealed envelope containing the writer's card. I don't know who the writer is, of course; but, if he should be here or should hear of this fact, he might send his card in sealed envelope,— send it to me if he will. Otherwise, when this question is determined, we might be unable to give the writer's name.

Are there any further remarks to be made? If not, a motion to adjourn would be in order.

On motion the Association voted to adjourn.

# PUBLISHERS' DEPARTMENT AND BOOK NOTES.

In this department the publishers will include notices of such subjects, germane to the scope of the *Journal*, which would seem to be of interest to its readers, but which are not a part of the transactions of the Association.

## A NEW IMPROVEMENT IN BURIAL CASKETS.

The problem of the manufacture of a burial casket which should be absolutely air-tight and fluid-proof has for a long time been the subject of much thought and not a little practical experiment. Such articles have been advertised heretofore, but probably every member of a Board of Health knows that there has been thus far no such casket readily obtainable which he could safely and completely indorse.

In the transportation of bodies over long distances and also in order that public funerals might be held over persons who have died of contagious disease, such an article is so essential that any progress in this direction cannot fail to interest the readers of this *Journal*. They will therefore be glad to learn that the problem now seems to be successfully solved, and that in a short time it may be possible to obtain a really air-tight and fluid-proof casket which Boards of Health can safely permit to be used under such circumstances.

These caskets which are being manufactured by the American Metal Casket Company are made from a special metal known as Leaded Sheet Steel, which, together with special machinery for their manufacture, has been invented by this company. These inventions were necessary, for never before has there been the need of stamping out with dies such large sheets of steel. The lead in the metal renders it malleable, and protects it from the action of fluids and from weathering, while its addition to the weight is trifling, the largest sized casket, completely fitted, weighing less than seventy-five pounds.

The top of the casket is therefore struck out of a single sheet of metal, and the body of four pieces. Added strength is secured by corrugation, and the only problem remaining to be solved is that of the sealing on of the cover. This, as in the case of so many essential inventions, is now provided for by a method so simple that it seems remarkable that it has not been discovered long before. There is no need of a plumber and his soldering-pot. A little tube of cement, a little pressure on some metal clasps, and the casket is so effectively sealed as to stand the severest tests.

These caskets are stronger and of lighter weight than those of ordinary construction. They will be sold at a moderate price, and cannot be distinguished in appearance from the highest grade of casket now manufactured, since they are capable of exactly the same finish and ornamentation.

They have already been shown to the officials of the War and Navy Departments, the Boston Board of Health, and the larger hospitals in this vicinity, and have met with unqualified approval; and the indications are that the support from these sources alone will tax the capacity of the plant for a long time. If the results prove as claimed, our readers can well believe this fact when they consider that the Boston City Hospital alone sends upward of four hundred bodies every year to the British Provinces alone. The members of this Association will soon have the opportunity of practical investigation, since, shortly after May 1, the company will have its representatives actively at work exhibiting the casket to the Boards of Health throughout the State.

# JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH

## April Meeting, 1900

SUBJECTS: Lead Poisoning as a Public Health Question — What shall Boards of Health do officially with Persons carrying Diphtheria Bacilli in their Throats or Noses without being ill, to prevent the Spread of the Disease?

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### North Andover.
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Charles B. Fowler.
W. H. Fullam.
W. H. Gove.
George G. Hathaway.
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Raymond L. Newcomb.
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### Wakefield.
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James C. Coffey.
Prof. L. P. Kinnicutt.
J. F. McCartney.
L. F. Woodward, M.D.

## THE JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

The Massachusetts Association of Boards of Health was organized in Boston in March, 1890, with the following objects: the advancement of sanitary science in the Commonwealth of Massachusetts; the promotion of better organization and co-operation in the local Boards of Health; the uniform enforcement of sanitary laws and regulations; and the establishment of pleasant social relations among the members of the Association.

All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

The Official Journal of the Association is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them. No part of this matter is printed in any other periodical.

The Journal will present, from quarter to quarter, a fair and adequate picture of the progress of practical sanitary science as applied to the needs of a modern community. The various subjects which are reviewed in the quarterly meetings of the Association are treated by experts qualified to speak from daily experience in Public Health offices, who, as men of science, are careful to be scientific and comprehensive, and who, as public officers, are no less careful to speak pertinently and so as to be easily intelligible to the layman.

The Journal, in a word, appeals to all whose interests touch the questions of sanitation and hygiene,— to the architect, the school-committee-man, the manufacturer, the contractor, and, above all, to the busy practitioner who has no time for any reading but what is brief and to the point.

The subscription price of the Journal is one dollar a year, payable in advance. Single numbers, twenty-five cents. It is on sale at the Old Corner Bookstore, and at Smith & McCance's Bookstore, 57 Bromfield Street, Boston.

All communications to the Association should be addressed to the Secretary, **EDWIN FARNHAM, M.D., City Hall, Cambridge, Mass.**

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# Massachusetts Association of Boards of Health.

*ORGANIZED 1890.*

[The Association as a body is not responsible for statements or opinions of any of its members.]

Vol. X. July, 1900. No. 2.

## APRIL MEETING

OF THE

## Massachusetts Association of Boards of Health.

The April quarterly meeting of the Massachusetts Association of Boards of Health was held in Boston at the Parker House on the afternoon of Wednesday, April 25, Dr. Samuel H. Durgin, Vice-President, in the chair.

The Chairman.— Will the meeting please come to order? The Secretary being unable to be present to-day, it becomes necessary to nominate some gentleman as Secretary *pro tem.*

Dr. Abbott.— I nominate Dr. Shea.

The motion was seconded and adopted, and Dr. Shea was elected Secretary *pro tem.*

The Chairman.— The Executive Committee have to recommend for membership in the Association the following names: —

| | |
|---|---|
| Dr. W. N. Sharp . . . . . . . . | South Framingham. |
| Dr. Horace J. Soule . . . . . | Winthrop. |
| Dr. Charles A. Willis . . . . | Waltham. |
| Dr. Marshall J. Mosher . . . . | Waltham. |
| Mr. R. S. Weston . . . . . . . | Brockton. |

| | |
|---|---|
| Mr. Ernest T. Badger . . . . | Providence, R.I. |
| Dr. E. L. Fisk . . . . . . . . . | Fitchburg Board of Health. |
| Mr. W. Lyman Underwood . . . | Belmont. |
| Dr. Edward Bancroft . . . . | Wellesley. |

These gentlemen were unanimously elected members of the Association.

The Chairman.— Unless there is some incidental business to come before the meeting, you will listen to the records of the last meeting by the Secretary.

Dr. Shea, Secretary *pro tem.*, then read the records of the January meeting in Boston.

The Chairman.— Any corrections to be made in the records of the last meeting?

Dr. Woodworth.— The name "Woodworth" instead of "Woodwich."

Dr. Shea.— Corrected.

The Chairman.— Any corrections? If there are no other corrections, the records will stand as approved. The next business on the programme is the reading of a paper by Dr. S. W. Abbott on "Lead Poisoning as a Public Health Question."

## LEAD POISONING AS A PUBLIC HEALTH QUESTION.

The management and control of infectious diseases undoubtedly constitute the foremost duty of sanitary authorities, and the greater share of attention of such authorities is very properly devoted to this department of sanitary work. But this does not prevent the exercise of due care in investigating and suppressing other causes of disease and death, which may prevail more or less frequently in large communities of people.

There are many sources and causes of disease which are preventable, and yet are not of the communicable class. Among these is the class of metallic poisons, which are in common use for various purposes in some form or other. Tin, zinc, copper, mercury, arsenic,

lead, and antimony,— all these have poisonous compounds which act injuriously upon man when taken into the system. It is to only one of these that I shall ask your attention.

The subject of lead poisoning is one which is of more than ordinary interest to medical men, to engineers, to chemists, and to those who are interested in forensic medicine. It is by no means a new subject. In the reign of the Cæsars the architect Vitruvius called attention to the danger from the use of water drawn through lead pipes.*

I shall treat the subject in this paper under two heads: —

1. Lead poisoning as related to the subject of industrial hygiene.

2. Lead poisoning from the use of lead pipes for the conveyance of drinking water.

And first, as to the symptoms of chronic lead poisoning, for it is with chronic poisoning that medical men have chiefly to deal. How may we distinguish between the symptoms of lead poisoning and those of other forms of poisoning, or even general symptoms not due to lead poisoning? In mild cases the diagnosis is often obscure, and there is no doubt that cases of illness are often attributed to lead which have no such origin.

In the majority of cases, nervous symptoms predominate. Pain is a very common symptom, and it is most common in the abdominal region. It is sometimes constant, sometimes remittent. The abdominal walls are frequently retracted at the umbilicus. Often there is nausea, and almost always constipation. The tongue is coated, thirst excessive, and the urine is usually diminished, sometimes albuminous. Severe pains in the joints and muscles are also common. After one or two attacks of lead colic, paralysis is a frequent symptom, and is most common in the forearm, and usually affects the extensor muscles of the forearm, hand and fingers. As a result, the hand drops when the arm is raised. Sometimes the muscles of the whole trunk and limbs are affected. This was the case with a young girl from Milford, Mass., who was for several weeks a patient at the Massachusetts General Hospital. In this case there was paralysis of the limbs with marked contraction, the legs being strongly flexed at the hips and at the knees. This was one of the

* De Architectura, book 8, chap. 6.

worst cases I ever saw, due entirely to drinking water through lead pipes, in that case.

Pallor of the skin is a very common symptom, and was noticed in nearly all of the cases which occurred at Kingston in 1895. There were about thirty-five of these cases in all; and I think I saw them all within two or three days, mostly in connection with Dr. Putnam. One of the most distinctive symptoms, however, and also one which is present in nearly all advanced cases, is the lead line upon the gums. The color of this line is distinctly different from the healthy pink color of the gums, and is usually from two to three millimetres in diameter, and shows itself most prominently at the base of defective teeth or old stumps. This characteristic mark was present in fully three-fourths of the Kingston cases.

In this book, which I will pass round and which is Dr. Thomas Oliver's book, probably the best modern authority in England in the present day, there are two plates showing the appearances of lead poisoning where it has affected the gums. These are fairly good. But it is almost impossible to get the delicate pink color of the gums unless great care is taken in the work of making the picture; and I don't think they have got at it so well in England, certainly, as we have here, in many instances.

Fatal cases of lead poisoning are not common, except in communities where lead industries are largely conducted, since severe cases are usually recognized; and the cause of the illness being discovered, and the proper remedy applied, the patient usually recovers. By the term "remedy" here I refer to the removal of the cause, and not to the therapeutic treatment of the patient.

One of the peculiarities of lead as a poison is its cumulative character, whereby extremely minute doses may be received daily into the system, and after a long period the effects become manifest all at once. Dr. Porter of the Royal Infirmary, in referring to the insidious character of lead poisoning among workmen, said that specific symptoms of such poisoning did not occur usually for thirteen or fourteen years, and that was the average time between the date of the first exposure of the poison and the first specific symptoms causing the workman to seek medical advice.

Dr. Thomas Oliver, one of the highest English authorities on lead poisoning, says of its effect on the system: —

The entrance of lead into the system in infinitesimal quantities, no matter the channel by which it gains admittance or the form in which it is absorbed, cannot go on without serious impairment to health. Thus do we seek to explain the rapid breakdown of the constitution of the lead-worker, the colic of the house-painter, and the extreme headache and anæmia of the lady of fashion who tries to deceive herself and her friends by obscuring the whitening touch of age by the use of certain hair restorers.—*Lectures before the Sanitary Institute, on Industries and Occupations, p.* 75, *London*, 1893.

Of this latter class of proprietary remedies, the State Board of Health in its Report of 1896 (p. 615) specifies eleven, which contained quantities of lead amounting in some instances to as much as $2\frac{1}{3}$ per cent. Instances of serious injury to health have been reported as due to the use of such preparations. The dishonesty of their manufacturers is shown in the use of the term "vegetable" given to several of them by their makers, with the evident intent to deceive the purchaser.

1. *Lead Poisoning from the Standpoint of Industrial Hygiene.*

There is scarcely any occupation in which man may engage which does not have some direct influence upon his health, either for better or for worse. The length of life of farmers and gardeners is longer than the average, chiefly because their occupations are conducted in the open air. Certain occupations and employments, on the other hand, tend to shorten the lives of those who are employed,—such, for example, as stone-cutting, knife and needle grinding and polishing, and all employments in which those employed are exposed to the action of irritating dust. Some time ago, in looking over the statistics of Massachusetts in regard to consumption, I came across the fact that one not very large town in Worcester County had an extremely high death-rate from consumption for a period of twenty years or more; and, not being sure as to the fact, I went down there and had some conversation with physicians, and found that the principal industry in that town had been the grinding of axes. That is the town of Douglas. This has nothing to do with lead, but simply as showing the effect of a man's occupation on his health, the irritating dust being drawn in by his breath. Employments in which

the workmen are exposed to the action of metallic poisons also shorten the lives of those who are engaged in them. I was once called to examine the condition of a factory where Paris green was made in large quantities in this State. Most of the operatives presented decided evidences of the injurious effects of handling and inhaling the fine powder which arose after the process of bolting was completed and the dried arsenical powder was ready for packing. Hardly anybody in the town would really work in the factory. It was almost always necessary to find people from outside, who were not familiar with the fact that it was such an injurious employment.

The number of occupations in which lead is employed in such a manner as to become injurious to the persons employed is quite numerous, especially in a State like Massachusetts, where the processes of manufacture are conducted to a greater extent than in any other State, when the ratio of persons employed is considered. I don't know but I may except Rhode Island. I can hardly imagine a more fruitful subject of legislative inquiry than this question of the effect of occupations upon the health of workmen, and the best methods of remedying such evils as may exist.

There is scarcely any definite information at present to be had in this country of a general character upon this subject. Physicians in active practice in the large cities occasionally report cases due to the effect of harmful occupations, and the records of general hospitals contain accounts of cases admitted to them; but very little systematic work has yet been done in the way of collecting this information so as to make it useful to others. We are therefore compelled to refer to the evidence offered by other nations than our own. In the reports of the Registrar-General of England the following occupations are noted as subjecting operatives to the injurious action of lead: lead-workers, file-cutters, plumbers, painters, potters, glass-makers, copper-workers, coach-makers, gas-fitters, locksmiths, lead-miners, printers, cutlers, and wool manufacturers. The British Parliament has directed that an investigation should be made with reference to those occupations which are specially harmful to persons employed in them; and several reports of the commission appointed for this purpose have already been published. Lead industries form an important topic of investigation in these reports.

Dr. Littlejohn, of Sheffield (health officer of that city), reported that ninety-one persons had died in that city from lead poisoning,— not simply cases, but deaths,— fifty-six of whom were file-cutters.

This matter of file-cutters I looked up, to see how that should have anything to do with lead poisoning. Here is a common file. When the workman wishes to cut the file,— it has to be cut with a sharp steel instrument,— it is necessary that it should be put upon something that will not injure the under side when that is cut. Of course, nothing hard, so a lead base is used. More than that, the lead base is a very inelastic metal. It does not bounce. It does not cause the file to bounce, as you might say, but allows it to rest absolutely dead upon it. In doing that with, of course, a great many files a day, there is constant attrition and rubbing away of the dust of the lead; and the workman's hands and face are smeared with it. He goes to his lunch; and of course, every day, if he is not absolutely clean, he swallows a small dose of lead. The powder is also, probably, to some extent inhaled in the air.

Among the occupations investigated were the manufacture of metallochrome powder, the use of lead in dye and print works, and the licking of labels in thread mills.

Very much can be accomplished in the line of prevention in such industries. First of all is absolute cleanliness, and thorough washing of the hands on leaving work. Hot water, soap, towels, and nail-brushes should be provided for all operatives, also respirators and overalls for all persons employed in any processes. Fans or ventilators should be provided wherever lead dust is generated. No food should be eaten in any part of the works.

The following drink is recommended: —

| | |
|---|---|
| Magnesium sulphate . . . . . . . . . . . . . . | 2 oz. |
| Water . . . . . . . . . . . . . . . . . . . . | 1 gal. |
| Essence of lemon . . . . . . . . . . | enough to flavor. |

Operatives are also recommended to keep a bit of alum in the mouth while at work.

In proof of the definite exposure of workmen to the action of lead are the investigations of Strumpf, who found lead in the dust of printing-offices.* The Austrian factory inspectors found $16\frac{1}{4}$ per

* *Hyg. Rundschau*, vii. 10.

cent. of lead in the dust of a type case, and smaller amounts in other parts of the room. Faber found similar results on ledges and shelves of rooms where similar industries were conducted.* Marcuse found lead poisoning in seven persons, in one day, who were employed in a factory for the making of bottle stoppers. A powder was used in one of the processes of manufacture which contained 30 per cent. of acetate of lead.

This came to our notice in a curious way some five or six years ago from North Adams. A physician in North Adams had lead poisoning, and wondered how it could possibly occur. He had no lead pipe in his house. But in the summer he was accustomed every day to take a drink of some sort of pop beer or lemonade,— I think every day, regularly, through the summer. He sent one of those bottles to us; and we found, I think, as high as 25 or 30 per cent. of lead in the stopper. The drink, being acid, of course had had some effect in dissolving that stopper into the material which he drank.

The following extracts from the last Decennial Supplement to the Registrar-General's Report (England and Wales) contain valuable information relative to this subject, and show the importance of investigation among those wage-earners who are subjected to the injurious action of lead: —

## EFFECTS OF CHRONIC LEAD POISONING.

A list of thirteen occupations in which, according to the returns for 1890–92, there is unmistakable evidence of poisoning by lead, will be found in the following table. For each occupation the mortality is shown, not only from plumbism, the mortality directly attributed to which cause forms by itself but an imperfect measure of the injury resulting from the absorption of lead into the system, but also from gout and from phthisis, as well as from diseases of the urinary, nervous, circulatory, and respiratory systems. The inclusion, however, of the six last-named diseases or groups of diseases must not be held to imply the belief that the excessive mortality which either of them may inflict on a given occupation is the result of lead poisoning, exclusively. Nevertheless, the figures, as they stand in the table, will be found instructive in connection with the present subject.

In the lowest line but one of the table the mean mortality figures have been inserted for the thirteen occupational groups taken together. These figures have been calculated in order to supply an average with which the mortality figures from various causes in the several occupations may be compared.

* *German Society of Pub. Health*, 1, 1898.

| | Plumbism. | Diseases of Urinary System. | Diseases of Nervous System. | Gout. | Phthisis. | Circulatory Diseases. | Respiratory Diseases. |
|---|---|---|---|---|---|---|---|
| Lead-worker | 211 | 161 | 232 | — | 148 | 272 | 397 |
| File-maker | 75 | 104 | 212 | 4 | 402 | 204 | 423 |
| Plumber | 21 | 81 | 131 | 13 | 165 | 123 | 218 |
| Painter and glazier | 18 | 83 | 132 | 9 | 232 | 147 | 225 |
| Potter | 17 | 63 | 123 | 1 | 333 | 227 | 668 |
| Glass-maker | 12 | 63 | 155 | 9 | 295 | 157 | 445 |
| Copper-worker | 8 | 60 | 85 | — | 294 | 186 | 406 |
| Coach-maker | 7 | 68 | 105 | 7 | 189 | 134 | 250 |
| Gas-fitter, locksmith | 6 | 50 | 108 | 5 | 223 | 104 | 205 |
| Lead-miner | 5 | 41 | 62 | — | 380 | 142 | 325 |
| Printer | 3 | 52 | 98 | 4 | 326 | 133 | 214 |
| Cutler | 3 | 56 | 91 | — | 382 | 167 | 518 |
| Wool manufacturer | 3 | 45 | 100 | 1 | 191 | 131 | 256 |
| Mean of the above | 13 | 66 | 117 | 5 | 248 | 146 | 287 |
| Occupied males | 1 | 41 | 82 | 2 | 185 | 126 | 221 |

On examining this table, it at once becomes evident that exposure to the risk of lead poisoning is associated with increased liability to disorders of the urinary and nervous systems. This is true of all those occupations which are specially exposed to that particular risk; and, further, it is found that those occupations which show the greatest excess of mortality from plumbism also show the greatest excess of mortality from diseases of the urinary and nervous systems. Among lead-workers the combined mortality under these two headings is represented by 393, and that among file-makers by 316, as against 123 only among occupied males in general. Among plumbers, painters, and glaziers the mortality ascribed to these causes, although less conspicuous, is still 75 per cent. above that among occupied males. These figures leave no room for doubt that the deaths which are definitely certified as due to plumbism constitute but a small proportion of the deaths really due to poisoning by lead among workers who are exposed to its influence. Careful study of the mortality from diseases of the nervous system among the large class of plumbers, painters, and glaziers, tends to the conclusion that the excess under this heading is very largely due to cerebral hemorrhage.

In several of the occupations subject to lead poisoning there is evidence of unusual mortality from gout, also. In other occupations, however (lead workers, for example), no death was ascribed to gout in the three years, 1890–91–92.

The combined mortality from phthisis and respiratory diseases is, on the

whole, excessive among the occupations now under notice; but its relative incidence suggests that it is mainly due to conditions of labor unconnected with the presence of lead. For instance, the workers who suffer most severely from these causes in the aggregate are potters or earthenware manufacturers, the next in order being cutlers, file-makers, and glass-makers. In all these cases the mechanical effects of inhalation of dust constitute a potent cause of the excessive mortality from lung affections. A further reason for doubting whether lead poisoning has commonly any great effect in increasing the mortality from diseases of the lungs is that some occupations in which an appreciable or even a large proportion of the deaths are due to the former malady are affected comparatively little by the latter kind of disease. For example, plumbers, who stand third in the list as regards their mortality from lead poisoning, are less liable to phthisis and to respiratory diseases than are occupied males in general; and painters, coach-makers, and wool manufacturers, all of whom appear in the list of trades subject to lead poisoning, sustain a mortality from phthisis and respiratory diseases combined which is in one case below that standard, and which in no case exceeds it by more than 13 per cent.

The circulatory organs, on the other hand, seem to be more directly influenced by the effects of lead poisoning, the mortality from diseases of these organs being above the standard by 62 per cent. among file-makers, by 80 per cent. among earthenware-makers, and by 116 per cent. among lead-workers. The connection between the two causes of death is not, however, invariable, inasmuch as plumbers and gas-fitters suffer less severely, and coach-makers, painters, and wool manufacturers suffer only a little more severely than the standard from diseases of the circulatory system.—*Supplement, Fifty-fifth Report of the Registrar-General of Births, Deaths, and Marriages in England*, Part II., p. c.

*Plumber, Painter, Glazier.*— Not fewer than 166,135 males above fifteen years of age were returned under this heading at the last census, the number having increased since 1881 by nearly 24 per cent. At the earlier age-groups the death rates of plumbers compare favorably with the corresponding standard rates; but, after their thirty-fifth year of age, plumbers die much more rapidly than do occupied males generally. As compared with the building trades as a whole, they suffer a mortality which is in excess at every age-group. The comparative mortality figure in this group of occupations is 1,120, and is, therefore, much above the figure for the building trades taken together. It also exceeds the standard figure for occupied males by 18 per cent. From lead poisoning these workers suffer severely, the deaths which are definitely returned under that head giving a mortality figure which is not less than nineteen. It is true that this is small when compared with the enormous mortality of lead-workers and file-makers from the same cause; but the two latter are the only occupations in which the mortality from this disease exceeds that of plumbers, painters, and glaziers. Separate examination of the returns relating to plumbers, on the one hand, and to painters and glaziers on the other, shows very little difference in the liability of these workers to lead poisoning. The difference, such as it is, is slightly unfavorable to

plumbers; but there are so many workers who follow both occupations that any comparison between the respective death-rates would be of doubtful utility. Lead poisoning is probably the primary cause of much of the excessive mortality of plumbers, painters, and glaziers from diseases of the nervous and urinary systems and from gout. From phthisis, from cancer, and from rheumatic fever plumbers die very rapidly; and their mortality from diseases of the circulatory system is also in excess of the standard for occupied males. The mortality figure of plumbers has fallen steadily and considerably since 1871, the fall having affected both divisions of the working period of life.—*Supplement, Fifty-fifth Annual Report of the Registrar-General of Births, Marriages, and Deaths in England*, Part II., p. lxiv.

With these facts before us, it seems reasonable to believe that an investigation of some of the industries of our large manufacturing cities where lead is largely employed might reveal conditions which are susceptible of relief by the application of proper measures for prevention,— conditions, too, which may possibly explain some of the vague, mysterious, and hitherto apparently unexplainable symptoms which present themselves occasionally in patients who had previously enjoyed excellent health,— conditions which occasionally send the patient away from his faithful family physician into the hands of the empiric, the Christian Scientist, or some other of the thousand and one methods employed for imposing on the credulity of mankind.

### 2. *Lead Poisoning from the Use of Lead Pipes for Conveyance of Drinking Water.*

I come now to the other topic, the question of lead poisoning in connection with the use of lead for the conveyance or storage of drinking water. As I have already stated, this subject is by no means a new one, since the custom of conveying water through lead pipe dates from an earlier period than the beginning of the Christian era; and the danger from this source was well known even at that early period.

Before the introduction of water into cities and towns by means of aqueducts came into general use, wells and cisterns were largely employed; and, where the old oaken bucket or the chain-wheel were in common use, lead poisoning was unknown. But many houses were supplied by means of lead pipe of one or two inches in diameter for

conveying water from the well to the pump in the house; and, in such cases, lead poisoning has occasionally occurred among the inmates of houses using such water.

Within the past fifty years the public water supplies have increased in the United States with marvellous rapidity. The number of cities and towns supplied with public water in 1850 was only eighty-three, but the number thus supplied at the present time has increased to nearly five thousand.

The water of these supplies differs very much in quality, the purest waters being generally those of the mountain, lakes, and streams, taken at points far above the possibility of pollution by the sewage of towns, while others are taken from rivers and lakes at points where sewage must necessarily mingle with the water supply.

There are also other natural differences in the character of the waters used for drinking, which depend on the character of the soil and surfaces from which they are collected; and this has more to do with the question of lead poisoning than the sewage matter, for there is no evidence that sewage has any particular connection with it,— not any direct connection. So that some waters may yield almost no solid residue, and are almost as pure as rain or distilled water; while others, and especially those which are collected in limestone regions, are excessively hard, and contain a comparatively large amount of dissolved mineral constituents. They also differ much in the relative amounts of ammonia, of chlorine, of iron, lime, and other elementary substances which they contain; and in view of these facts it is not surprising that some of these many waters should exercise a solvent action upon lead. The public supplies of several English cities have been of such a character as to produce serious outbreaks of lead poisoning among the inhabitants dependent upon the public supply. This was notably the case in Sheffield, where the number of victims was so great as to lead to an investigation by the Local Government Board.

As a result of this investigation, the board concluded that the solvent quality of the water was due to acidity. This water, in the case of Sheffield, was collected from water-sheds in which there was an unusual amount of peaty land.

Many experiments were made by the board upon waters obtained

from moorland districts in Yorkshire, Lancashire, and other counties in which several million inhabitants were supplied with public water; and the only characteristic which appeared to be constant in its relation to the solvent action of the water upon lead was the presence of acidity derived from the peat. It was furthermore stated that, "while neither moorland water nor a sterile decoction of peat can of itself develop acidity, the addition to either of a minimal amount of moist peat soil will cause bacterial growth in it, with increasing development of acid reaction, and ability to dissolve lead." I don't know that this has ever been confirmed anywhere except in the English experiment, that perhaps Professor Kinnicutt can tell us about. I certainly hope he will.

In Massachusetts attention was called to this subject as early as 1842 by a report of a committee of physicians of the city of Lowell appointed to consider the subject, and to report upon it. There appears to have been a peculiar quality of the ground water at Lowell which acted unfavorably upon lead pipe, and it is remarkable that in the recent experience of that city the earlier investigation of its municipal government had been forgotten.

Under what conditions does the use of lead pipes or cisterns cause harm to those who use the water conveyed or stored in them?

1. *The Quality of the Water.*— Mr. H. W. Clark, chemist of the State Board of Health, has treated the chemical side of this subject in the last report of the board. I was very much in hopes that he would be here to-day. I understood he would be. He shows there that the waters of the State differ very much in the degree of their solvent power, from waters which take up less than $\frac{1}{100}$ part per 100,000 to a maximum in one instance of 8. per 100,000. The places where lead poisoning has occurred in the State use ground water at the present time, and in some instances no cases of lead poisoning had been observed until a change had been made from surface to ground water.

In only a few of the large cities and towns of the State is ground water in use,— a fact which may account for the comparative rarity of the occurrence of this form of poisoning. Generally, you know, it is easier to get water by storage in large ponds and lakes and draw it from streams than it is to pump it from driven wells.

In the general report of the State Board of Health for the same year (1898) — that is, the last report — it is stated that "the exact amount of lead which may be taken into the system without producing harm is not definitely known, and may vary with different people; but it is known that the continuous use of water containing quantities of lead as small as .05 of a part per 100,000, or about $\frac{1}{33}$ of a grain per gallon, has caused serious injury to health." See what an extremely minute amount that is,— only half of a millionth part of the water.

All ground waters do not attack lead in a similar degree, since experience has shown that only certain waters which have some peculiar quality are found to possess high solvent power; and this quality appears to be due to the presence of carbonic acid.

2. *A Second Condition which acts as a Factor in causing Danger to Consumers of Water is the Length of the Pipe.*— Other things being equal, a pipe 100 feet long would undoubtedly prove more dangerous than one of 10 or of 50 feet in length. In the town of Kingston, where by far the most severe outbreak of lead poisoning occurred, when the ratio of attacks to the number supplied is considered, several houses were situated at the end of long lanes or in large open lots of land, requiring long lines of pipe,— in one instance, 700 feet. In this instance the pipe supplied three families. In one of the families on this long line of pipe there had been no cases of illness, and on inquiry it appeared that this family were in the habit of allowing the water to run a long time before using it for drinking. At the house at the extreme end of the pipe there had been several severe cases of illness, and on inquiry the statement was made that they had usually drawn a pailful of water before taking any for use. But this pipe in its 700 feet of length contained several pailfuls of water, so that it would be necessary in the morning to draw at least three or four pailfuls before it would be possible to get water which had not stood in the pipe over night. It was not a very large pipe, I think,— perhaps it was one-half or five-eighths of an inch or more,— but not much larger than that.

3. *Method of using Water drawn through Lead Pipes.*— This point has already been discussed (see 2). The experiments of the board show, however, that a considerable quantity of lead is taken up by running

water; but, on the whole, water that has stood in a pipe for several hours will usually contain much more lead than water which has been run continuously through a pipe without being allowed to stand in it. This fact shows the necessity of caution in the methods of using water drawn through such pipes, when it has once been determined that the water exercises a solvent action on lead.

Bright new lead parts with its lead more readily than old pipe. There is one point I had not thought of here; and that is, of boiling the water. People in typhoid fever districts have been recommended to boil the water. Of course, it gives absolute immunity in that case by destroying bacteria; but in the case of lead it only makes the thing worse, because it concentrates the metal. You cannot kill the metal, and you cannot distill it all.

4. *Susceptibility.*— It is probable that some persons are more susceptible to the action of lead than others, reasoning from the action of other poisons in general. But this hypothesis is not sufficiently established to encourage any one to test the question with a view to establishing the fact of his own immunity or susceptibility to the action of lead.

What methods may be employed to prevent the occurrence of lead poisoning from the use of drinking water?

1. We have seen that the quality of dissolving lead is not common to all waters. It is therefore desirable, whenever a new supply is introduced into a city or town, to ascertain by experiment whether or not it possesses this property of dissolving lead. With a single sample of two or three gallons it would not cost much, and it might save a good many people from harm and danger. So far as the State of Massachusetts is concerned, this proposition applies with special force to those places where ground waters are proposed for use as public supplies. The fact of such solvent power or quality having once been determined, a new source may be sought for yielding a water free from this objection.

2. The use of lead pipes and cisterns may be abandoned or altogether forbidden: I mean in public use. Of course, you cannot forbid a man to use lead in his own house, I imagine. It is not like a contagious disease. By becoming poisoned with lead, he does not communicate it to others. So that you have a right to go into a man's

house in case of contagious disease and perhaps do things, but I doubt whether you would have a right to enter his house and pull out lead pipes. The city or town authorities have undoubted right, however, to act upon all pipes in their possession up to the house limits.

3. Instructions may be published and circulated among the people, cautioning them as to the use of the water and recommending that it be allowed to run freely before being used either for drinking or for cooking. Such instructions, however, are liable to be disregarded. Moreover, it has been found that appreciable quantities of lead may be dissolved by running water.

4. In a few places in England the plan has been adopted of treating the water at its source with chemicals (chiefly some compound of lime), to change the quality of the water, so that it shall not attack lead. This, however, is an expensive method, and requires constant supervision at the source of supply or at the distributing reservoir.

NOTE.— In a paper by Dr. C. H. Tattersall read at the Sanitary Congress at Leeds, England,— he came from one of the towns that had a large amount of lead poisoning, I think the town of Oldham — in September, 1897, the author mentions six different chemicals which had been subjected to experiment, with a view to using them for preventing the solvent action of water upon lead.

With reference to the prevention of the action of water from peat regions — that is, those where they have had the most trouble — upon lead he recommends the following measures: —

1. "That the water be got off the peaty gathering grounds as quickly as possible.

2. "That as much time as possible be given to subsidence and bleaching.

3. "And, most important, that the water be filtered through ordinary sand filters, or, failing this, be passed over 'Paris White,' as at Sheffield. [That is, I think, the finely powered carbonate of lime.]

4. "That block-tin-lined pipe of good quality be used for water service instead of ordinary lead pipe."—*Journal of Sanitary Institute*, vol. 18, 1897, p. 625.

THE CHAIRMAN.— Dr. Abbott's paper is before the Association for discussion. I take the liberty of calling upon Professor Kinnicutt.

PROFESSOR KINNICUTT.— Lead is not acted upon by pure water in the absence of air, but distilled water containing air forms a hydrate of lead which is soluble in water; and experiments made during the past

year by the Massachusetts State Board of Health have shown that, when distilled water is saturated with air and allowed to remain in contact with lead, a gallon of this water will contain about one grain of lead.

As to the natural waters, their solvent action on lead is influenced by the presence of various dissolved substances in water. Thus water containing ammonium salts and nitrates acts rapidly on lead pipe; and the same is true of waters containing free carbonic acid. Hard waters, on the other hand, containing sulphates and carbonates, have very little action. Thus it is that there is more danger of action from contact of pure water with lead than from impure water, a water containing a large amount of mineral matter having comparatively little action. This is undoubtedly due to the fact that the inside of the lead pipe becomes coated over with unsoluble salts of lead, thus preventing further action of the water.

Water from peat districts, as Dr. Abbott has said, often has a decided action on lead. This is due to the fact that such water contains very little mineral matter, and that the organic vegetable matter it does contain is oxidized, giving, as Mr. Clark has shown, free carbonic acid.

There is another point mentioned by Dr. Abbott, as regards the action of sewage on lead, that I think might be misunderstood. Fresh sewage has certainly little action on lead, but, when the sewage undergoes decomposition, the nitrogenous organic matter is changed into nitrates and carbonic acid; and thus in water which has been polluted by sewage we find both these substances. Consequently, such water acts easily on the lead.

It has been called to my attention lately that certain distilled waters which are on the market contain lead. This may sometimes be due to the use of lead pipes in the condensers. In one case which I recently investigated, all the pipes were of block tin. There were, however, many soldered joints; and the action of the distilled water on these soldered joints was sufficient to cause the distilled water to contain marked traces of lead.

There is one point that may be interesting to some of you; and that is, how easy it is to detect any notable quantity of lead in the water. All that it is necessary to do is to take the water and place

it in a white bowl or a white porcelain dish, and add to it a drop or two of sulphide of ammonia, which is very easily obtained from any apothecary. If there is any iron, lead, or copper in the water, the water will turn dark, having a darkish appearance against the white porcelain. This may be caused by iron as well as by lead, or by copper; but, if we add to the water which has just turned dark a drop of hydrochloric acid, the color will disappear if it is owing to iron, while, if it is owing to lead or copper, it will remain.

To make sure that there is a darkening of the water, we place in another white porcelain dish about the same amount of pure distilled water and add to that a drop of sulphide of ammonia, and then compare the color, to see if one is darker than the other. And if one is darker than the other, and that darkness does not disappear by adding a drop of hydrochloric acid, we are sure that either lead or copper is present in the water tested; and such water is not to be used until it has been further investigated, and the amount of lead or copper it contains determined.

As regards lead poisoning which comes from the manufacture of white lead, the manufacture of files, etc., I thoroughly agree with Dr. Abbott, and believe that the legislature of Massachusetts should appoint a committee to investigate the dangerous trades of the State, and see what precaution should be recommended for the protection of the workmen.

THE CHAIRMAN.—Shall we have a word from Dr. Hill?

DR. HILL.—As I am a bacteriologist rather than a chemist, I would rather hear from the chemists on this point.

THE CHAIRMAN.—Dr. Morse, of Lowell.

DR. MORSE.— It is a little over a year ago since I began the study of lead poisoning under the direction of the State Board of Health; and the clew for my study was furnished by Mr. Clark, the chemist of the board. He prepared a list of cities and towns in which it was stated that their public water supply, experimentally, dissolved a certain amount of lead when it came in contact with lead pipes. I accordingly investigated twenty of these cities and towns, and found a total of over 130 cases of lead poisoning distributed among them.

The four principal places were Lowell, Milford, Fairhaven, and Kingston. At Lowell over 70 cases of lead poisoning were found, all of which had developed within the preceding five years. It became apparent that, as the physicians and people at Lowell were made cognizant of the existence of the disease, cases were found more readily after a preliminary study than had been the case previously; for, after a study covering perhaps six or eight weeks, about forty cases of the disease had been discovered, while in the succeeding three months, among one-fifth of the physicians of the city, 33 new cases had been stated to exist. At Fairhaven, where the disease had been previously reported, no new cases had developed. This is probably due to the advice which the State Board of Health had given them, in which all of the inhabitants were advised to let their water run for a certain length of time before using the water for drinking purposes. At Kingston, as Dr. Abbott has said, a number of cases had existed; but, acting also on the advice of the State Board of Health, some of the lead pipes had been removed. The people, in addition, had been particular about drinking water which had stood for a certain length of time in the pipes; and no new cases appeared. At Milford, however, 30 new cases were discovered; and the board again advised the Water Board of Milford and the local Board of Health of the danger that existed in drinking the water which came through lead pipe. Some of the older cases of lead poisoning at Milford were also seen, and they had almost unanimously shown considerable improvement. The girl that Dr. Abbott refers to was seen; and, by means of an operation which had been performed upon her extremities, they have nearly regained their normal position, and she was very much better in a general way than she had been previously. Dr. Abbott has truly stated that some physicians call cases lead poisoning, when apparently that is the wrong diagnosis. I can also state that the reverse of that is true,—that some doctors do not recognize cases of lead poisoning. This was plainly shown to me in two of the most marked cases which I had the opportunity to observe at Lowell, in which a father and his daughter were seriously ill with the disease. They both had constipation, headache, a marked lead line on their gums, as marked as is present in those plates which we have just seen in the books. The

doctor who attended them pronounced the case gastritis. They continued drinking the water; and, eventually, they were so debilitated by the disease that it was necessary for them to go to bed. They became discouraged with his treatment, and, changing doctors, finally procured one who recognized the disease, immediately placed them upon proper treatment, and the last time I saw them they were on the road to recovery. No two plainer cases of lead poisoning could exist than those which this doctor had evidently failed to discover. It is also a significant point that some of the people in Lowell who had been affected with the disease, upon going away, temporarily improved, and their symptoms subsided; but again on their return the symptoms returned to them upon drinking the water, and they became worse than they were before. There was a certain class of three or four patients in Lowell which had the disease, and this was the only single class which I could differentiate from the others. They were three or four policemen who were employed by the city. I sought for the cause of their infection at the police station. At that place there was an ordinary water tank, which was supplied by the public water supply of the city; and the water was brought through a lead pipe some one hundred feet to the tank, and a faucet directly over the tank permitted the tank to be filled whenever it was necessary. The janitor of the building stated that it was customary for him to draw a certain amount of water before filling the tank. I suppose he did; but he evidently did not draw a sufficient amount, because three or four of these policemen were affected with the disease, one of them quite seriously.

The Chairman.— Are there any other remarks to be made upon this paper?

Dr. Hill.— I should like to ask Dr. Morse how often he found families in which lead poisoning occurred, but only one member of the family affected; and, in such cases, if he could find any indications of any sort of immunity in the other members.

Dr. Morse.— I would answer Dr. Hill's question by referring to the two cases which I have previously spoken of,— the father and daughter who lived at Lowell. They were at home all the time, while two other children, both boys, worked in one of the mills down town,

where evidently they were not so seriously exposed to disease as the father and daughter were at home. These boys had no symptoms of the disease. They were only at the house at night, and the chances are that they drank very little water at their homes ; but the father and daughter, who remained at the house all the time, were seriously ill with the disease.

Dr. Hill.— Are there any cases, doctor, where all the members of the family drank the suspected water in about the same proportions, and yet only one member was affected, the others escaping?

Dr. Morse.— No definite effects.

Professor Kinnicutt.— There is one curious fact that I might mention, which is that Dr. Wood of the Medical School told me that he had had a number of cases where horses had been poisoned by lead, and with very serious results. Of course, in most of these cases it was traced to lead troughs.

As regards Dr. Hill's question, I might mention that I have had at least three cases, where water was being used containing notable quantities of lead, in each of which cases only one person out of a number of persons using the water was reported as suffering from lead poisoning.

The Chairman.— As a matter of interest, I am tempted to ask Professor Kinnicutt to mention some of the principal symptoms of lead poisoning noticed in the horse.

Professor Kinnicutt.— I think the question can best be answered by medical men. I examine a water, but do not pretend to decide if the persons using the water are suffering from lead poisoning.

The Chairman.— Are there any other remarks upon this paper? Perhaps we have devoted as much time as the requirements of the next paper will allow. I would therefore, for the committee appointed by this Association some time ago to receive papers written for a prize and to award the prize, state that the committee has done its work, and has awarded the prize to X. Y. Z. Whoever this may be, his name is sealed in an envelope, which seal will be broken as soon as the paper has been read by Dr. Brough, of Boston.

Dr. Brough.— Mr. Chairman and gentlemen, I shall read this paper, which was written by a gentleman whose name I do not know, on the subject, "What shall Boards of Health do officially with Persons who are carrying Diphtheria Bacilli in their Throats or Noses without being Ill, to prevent the Spread of the Disease, and how can these Bacilli be most rapidly destroyed?"

## PRIZE ESSAY.

### Part I.

*What shall boards of health do officially with persons who are carrying diphtheria bacilli in their throats or noses without being ill, to prevent the spread of the disease?*

The first step in an attempt to solve this problem is to determine to what an extent these cases are a source of danger. What is the significance of the report of a bacteriologist that a culture made from a healthy throat contains the bacilli of diphtheria?

The diagnosis of the cultures sent to our Board of Health Laboratories is based solely upon the morphological appearance of the bacilli. A culture containing bacilli which look like Klebs-Löffler bacilli is reported to be positive without any further study of the bacillus. No further study is possible where a report of a culture must be made within twenty-four hours. Nor is any necessary in ordinary cases; for it has been shown by many investigators that both in cultures made from cases of clinical diphtheria, and also in simple sore throat or tonsillitis, bacilli, which morphologically are identical with the diphtheria bacillus, are, with very few exceptions, true virulent Klebs-Löffler bacilli.

With the bacilli found in cultures from the healthy throat the conditions are somewhat different. Here, while in some cultures highly virulent bacilli are found, in others there are bacilli corresponding in every way to Klebs-Löffler bacilli, excepting that they are not virulent. Again, there are other cultures containing bacilli which may morphologically resemble the diphtheria bacillus, but differ, not only in an entire absence of virulence, but also in growth on certain media, the most important of which is the failure to produce acid in sugar bouillon.

The frequency with which diphtheria bacilli are found in healthy throats varies greatly. Among the well-to-do it is probably very uncommon, except in those who have been exposed to diphtheria. Among those living in crowded tenements, where the disease may almost be said to be endemic, also in children's institutions where the disease has been prevalent, healthy individuals, containing Klebs-Löffler bacilli in their throats or noses, are not uncommon. Some of these cases show bacilli without virulence, while in others the bacilli are highly virulent.

Virulent diphtheria bacilli are found frequently in the throats of individuals who have been exposed to diphtheria. Park * made cultures from forty-eight healthy children in fourteen families where all had been exposed. 50 per cent. of these cultures were positive. The virulence was tested in six of these, and all were found to be virulent. These were families living in tenements in New York, where the opportunities for infection were very great. 40 per cent. of the children later developed the disease.

Cultures are seldom made from the throats of healthy individuals unless they have been exposed to diphtheria. Consequently, in our Board of Health Laboratories most of the diphtheria bacilli which are found in cultures from healthy throats will prove to be virulent.

The occurrence of virulent Klebs-Löffler bacilli in the throats of healthy individuals, who never develop the disease, is best explained by the assumption that certain individuals have a natural immunity. Wasserman † and others have demonstrated in the blood of persons who have never had any throat affection a certain amount of antitoxin. They find that the blood of certain individuals have this antitoxic property to quite a marked degree, while others have less or none at all. The theory, therefore, of Wasserman and others, that the natural immunity which certain individuals possess is due to the antitoxic property of their blood, seems more than probable. On account of this immunity the bacilli multiply in their throats without producing symptoms, just as they do in the throats of those convalescing from diphtheria. In convalescence there is a temporary *acquired* immunity, due to the accumulation of antitoxin in the blood

* Park, "American Text-book Practical Medicine," vol. i. p. 647.

† Wasserman, *Zeitschrift f. Hygiene*, 1895, xix. 408.

during the course of the disease. This protects the individual from the further development of the disease, even though virulent bacilli may continue to multiply in the throat for weeks after the disappearance of all symptoms.

The condition of a healthy individual with virulent Klebs-Löffler bacilli in the throat is practically the same as that of a convalescent from diphtheria. The former has a *natural*, the latter an *acquired* immunity. The bacilli may be present in each in about equal numbers. They may at times be more abundant than they are in some cases of diphtheria during the acute stage. I have seen an almost pure culture of virulent diphtheria bacilli from the throat of an individual who had never shown a symptom of the disease.

We know the bacillus of diphtheria to be the specific cause of the disease. Other things being equal, a case is dangerous in proportion to the number of bacilli which are given off from it. The bacilli are disseminated just as much from the individual who has never had any symptoms as they are from those convalescing from diphtheria; and, consequently, they are just as much a source of danger. There are many cases reported in the literature where the infection can be traced with certainty to cases where there have never been any symptoms. Convalescents from diphtheria are isolated until the bacilli have disappeared from their throats, and no intelligent person questions the wisdom of our boards of health in so doing, Ought any different course be pursued with those other equally dangerous cases?

Let me briefly sum up the most important points bearing on the question in hand which have been mentioned.

Diphtheria bacilli in cultures from a healthy throat may or may not be virulent. The bacilli from the throats of those who have been exposed to diphtheria are usually virulent. These cases correspond in regard to the number of bacilli present, and also as sources of infection to cases of diphtheria where the bacilli persist in the throat after the acute symptoms have subsided. These convalescent cases are always isolated until the bacilli have wholly disappeared.

In view of these facts I think the following conclusions are justified: —

*First.* Healthy individuals with diphtheria bacilli in their throats

ought to be isolated until it is determined whether or not the bacilli are virulent.

*Second.* If the bacilli are found to be *non*-virulent, the case may be released.

*Third.* If the bacilli are virulent, the isolation should be continued and the case considered as one of diphtheria, subject to the orders of the board of health in regard to that disease.

These measures should be carried out in practice as follows: —

When the bacteriologist has found diphtheria bacilli in a culture from a healthy throat, he should report the fact to the board of health. A representative of the board (agent or inspector, as the case may be) should at once see that the individual is properly isolated. This isolation should be as complete as possible, even to the extent of sending the patient to the diphtheria hospital, if, in the judgment of the agent or inspector, the isolation could not properly be carried out at home.

If the bacilli are found to be non-virulent, the case may be released from quarantine; for in the present state of our knowledge it seems very improbable that these cases are in any way a source of danger. It would seem wiser, however, not to allow a child to return to school until the bacilli have disappeared; for there is a possibility that, although the bacilli which were isolated were non-virulent, there might be others in the throat which possess some virulence. We also cannot say with certainty that these bacilli do not, under certain circumstances, become virulent.

If, on the other hand, the bacteriologist reports the bacilli to be virulent, the isolation of the individual ought to be continued, and the case considered and treated by the board of health just as a case of diphtheria.

Difficulties at once arise if any other course is pursued. Supposing we were to consider as diphtheria all cases of sore throat in which the diphtheria bacilli were present, and to insist, as we now do, on their isolation, while healthy individuals with the bacilli in their throats are allowed to go free. How are we to decide what constitutes a sore throat? How much inflamed must the throat be? In this climate there are many individuals whose tonsils are large, and the mucous membrane often reddened. Shall such individuals be isolated, when

by chance, after exposure, diphtheria bacilli are found in their throats, while other individuals with an equal number of bacilli, but with healthy-looking mucous membranes, are allowed to go free?

Or shall we depend on what the individual tells us? Those who complain of sore throat are to be isolated, those who do not go free. I fear that, when people learn that this distinction was to be made, many of those in whose throats diphtheria bacilli were found would be unwilling to admit that there was anything the matter with their throats.

The following case, which came to the attention of the writer a short time ago, illustrates the difficulties which a board of health would meet, were any distinction to be made between cases with symptoms and those without: —

Two cases of diphtheria occurred in the family of a milkman. Cultures made from all the well members of the household revealed the fact that one of the hired men had a great many diphtheria bacilli in his throat. (These were later found to be highly virulent.) The man (an Irishman), apparently of average intelligence and morality (for his class), said that he had not been sick at all during the past month and had not had a sore throat. However, in accordance with the custom of the local board of health, the man was sent to the diphtheria hospital, as he could not be properly isolated at home. A week or two later it was learned from a small child in the family that the man really had been sick in bed with a sore throat a short time before the bacilli were discovered. Supposing that this man had not been isolated in view of his statement that he had had no sore throat. Consider what a source of danger to the public he would have been. Consider also what an injustice that this man, who had lied, should have his freedom, while there were others in the community at that time who were kept quarantined, although on account of the small number of bacilli present in their throats they were less a source of danger than he.

The principal arguments which are brought against the isolation of these individuals are based on the assumption that it is not proven that the Klebs-Löffler bacillus is the specific cause of the disease, Such arguments can be dismissed without comment in a communication addressed to the Massachusetts Association of Boards of

Health. It is urged that, because the bacilli do not give the disease to the individual in whose throat they are growing, they will not give it to another individual. The theory of Wasserman and others, however, that certain individuals have a natural immunity dependent on the presence of antitoxin in their blood, sufficiently explains this apparent inconsistency. Moreover, the many authentic cases reported, where infection can be traced with certainty to such persons, proves beyond a doubt that all individuals with virulent bacilli in their throats are a source of danger.

In an address at a meeting of the Deutschen Verein für Oeffentliche Gesundheitspflege at Kiel in 1896, Fraenkel * made an earnest and eloquent plea for the isolation of all cases in which diphtheria bacilli are present. He said: "We must not only isolate those with true diphtheria and those with simple tonsillitis, but also those individuals who have never shown any symptoms. An impossible demand, most of you will say; and, indeed, I know that at this time the carrying out of this to the letter is not to be thought of. But for this reason the more I feel it to be my duty to put myself thus on record, and to hold this up to the medical profession as the ideal, the goal toward which we must constantly strive."

At the time this was said (1896) there were no municipal or State laboratories in Germany where free bacteriological examinations of cultures were made. Under these circumstances it must have seemed well-nigh impossible to secure the detection and isolation of these cases. In this State, however, where we have State and municipal laboratories, the isolation of all of these cases which are detected can be carried out.

The board of health with which the writer is connected has isolated six of these cases during the past six months,— three in the diphtheria hospital and three in their own homes. All the orders of the board were complied with, just as in a case of diphtheria. Objection to the action of the board was made in one case; but the board had, very generally, the support of the public.

People will very soon get to understand about these cases; and when they realize that the danger of infection does not depend upon the severity of the symptoms, but on the presence of the bacilli in

* See Fraenkel, Berlin *Klin. Wochenschrift*, 1896, 33, 892.

the throat, they will support the board of health in its efforts to stamp out the disease. On the other hand, our boards will be justly censured if the disease is found to have been spread by individuals who were known to have the bacilli in their throat, but who had not been isolated.

## Part II.

*How can these bacilli be most rapidly destroyed?*

The answer which must be given to this question is that, at the present time, we do not know of any special measures which hasten the disappearance of the bacilli. The problem of how to destroy the bacilli in the throats and noses of those who have not been ill is practically the same as that of destroying the bacilli in those convalescing from diphtheria. Probably no branch of therapeutics has been more thoroughly investigated than this. In every contagious hospital in the civilized world various methods of treatment have been tried. Every physician who sees many cases of diphtheria has tried different remedies. It is an especially favorable subject for systematic investigation. There is something definite to be accomplished, and you have in bacteriological cultures an accurate means of measuring the results of treatment. Yet, in spite of this, there are to-day no considerable number of experienced men who believe that any special line of treatment will hasten the disappearance of the bacilli.

In view of all the work that has been done, it seems improbable that we shall ever be able to accomplish much with the use of antiseptics locally. Almost every one is agreed that strong antiseptics are to be avoided. Mild solutions which serve to tone up the mucous membrane and remove the bacilli mechanically are desirable, but no very marked effect is to be expected from their use.

The bacilli are very persistent in those who are living under unfavorable conditions, especially among children who are crowded together in institutions. This suggests the importance of general hygienic measures,— good food, a large air space, sunlight, and an opportunity to get into the open air on warm, sunny days.

Beyond these few general measures there is not much to be done for treatment. We can, however, do a great deal in the way of proph-

ylaxis. The more thorough we are in our isolation in diphtheria, and the more of these individuals without symptoms we detect and isolate, the fewer of these cases we shall see.

The Chairman.— I will now open the sealed envelope which contains the card of the author of this paper (opening the envelope, and reading the name enclosed). Dr. Francis P. Denny, of Brookline. [Applause.] The paper is now before the Association for discussion. Dr. Mason, of Fitchburg.

Dr. Mason.— Mr. President, I am very glad to hear this paper, even more than I am for the dinner. The reason why I am particularly interested in this subject is because in Fitchburg we have been going through a little experience of just this kind of thing. I wanted to see if I could get some information from this essay, and I have. Perhaps it would interest the Association if I gave a brief account of what we have been through in Fitchburg, though it turned out to be a rather small affair.

It seemed to start out as though it was going to do business. In the beginning, I want to say one thing, which is that I think we were negligent last fall in not taking more cultures than we did. In the family of a milkman in West Fitchburg last September there was a case of diphtheria in a child. It was a very virulent case, and proved fatal within three or four days. The regulations of the Board of Health were carried out to the letter, and everything necessary was done. The child was isolated properly, and thorough fumigation and that kind of thing occurred afterward; but I regret to say that no cultures were taken from the other members of the family at that time, and I think that is where we made a mistake. Nothing further was heard from this milkman until early this month; and in the first week of April, the first ten days of April, ten cases of diphtheria developed upon the route of this man, who supplied milk to a small milk route. We learned very early that he supplied the milk, and he was immediately shut off from carrying any more milk; and a sample of the milk was obtained for analysis, to see whether there were any diphtheria bacilli in it. That is a kind of work that I am not as well versed in as I might be. I failed to find any bacilli in the sample of

milk that was sent me. But I took cultures from all of the family of this milkman, including the milkman himself and four other individuals; and I found that four of them were positive. One, the case of the man who peddled the milk, was very positive; that is, it grew immediately, a very vigorous growth, within fifteen or eighteen hours. The other three cases were not so vigorous. At the first examination I did not find any bacilli, excepting in this case of the man who carried the milk about; and, as these cases did not develop until the first of April, my theory was that this man was the source of contagion himself, and not the milk, because I don't see, if bacilli got into the milk, why they should not have gotten in before and caused these cases to appear earlier, because I have no doubt that some of the other members of that family have had diphtheria bacilli in their throats for I don't know how long, it may be months. It seems to me it is a very clear case of infection by a man who apparently was perfectly healthy. When I went up to take the cultures, they laughed about it, said they were all right, nothing the matter with them at all. There was no appearance of anything being the matter with them, and I doubted myself whether I should find any bacilli. But, as I say, I found four out of five cultures positive.

I am very glad to have learned what I have from this essay, and I think that we shall know better how to act in such a case in the future than we have in the past.

The Chairman.— Will Dr. Chapin, of Providence, say a word in this discussion?

Dr. Chapin (Providence).— Mr. President, I, too, was very much interested in this subject. It seems to me to be one of very great importance, and certainly the writer of the paper took an entirely logical position. Whether or not the plan which he outlines can be carried out by health officers is another question, and I do not feel at all certain in regard to it. I think we shall all agree that the logical treatment of a tuberculosis subject, at least so far as it concerns cattle, is that every tuberculous animal should be isolated, and the most of us think that the best form of isolation is found in the rendering tank; but certainly the attempt to carry out those views has not been successful, and, I imagine, it will not be for many years. It is

questionable whether the isolation of every person who has diphtheria bacilli about him is any more feasible. It may be interesting to know what we have done in Providence.

For two years past we have tried to do this very thing, and have been pretty successful. We have been as successful in it as we are in isolating children who are desquamating from scarlet fever. We have examined the throat of every member of every family in which a case of diphtheria was reported, and wherever the diphtheria bacilli have been found in the throat those persons have been isolated. We frequently examine the throats of other children in the house; and, if the diphtheria bacilli are found, these children are isolated. For two years past we have had very little diphtheria in Providence, a city of 160,000 or 170,000 people. In 1898 we had 30 deaths, and in 1899 36 deaths. It might be argued that we were doing very good work. I very much doubt it. I very much doubt whether what has been done has had any appreciable effect upon reducing the diphtheria, because I don't believe we have begun to isolate all the cases which have diphtheria bacilli about them. It seems to me that the crucial point in this whole discussion is, How prevalent, how widely distributed, are diphtheria bacilli? and that is a question which has not received the attention which it should have. We do not know how prevalent they are. If there are not very many persons with diphtheria bacilli present about them, they may be isolated; but, if there are hundreds or thousands, they certainly cannot be. And then, if we attempt to isolate a few and not the rest, isolate those that we can get hold of through the reports of conscientious physicians and not isolate those who go to a physician who won't report them and who won't take cultures, then we are holding a very illogical position, and we shall soon be called to account, not only by the physicians and the public, but by the courts as well.

We have not made very many examinations in Providence of healthy persons who are presumably not exposed to diphtheria; but some hundreds in the public schools have been examined, and we have found diphtheria bacilli present in a considerable proportion. If I remember right, Dr. Park's experiments in New York — the early experiments — showed that diphtheria bacilli were quite prevalent in that city. I saw in the last report of the Board of Health of Balti-

more that in that city last year some 4,000 school-children were examined, and diphtheria bacilli were found in the throats of 199. You can see that that is a pretty large proportion. At that rate, you would have probably three or four thousand children here in Boston with diphtheria bacilli in their throats, whom you would have to isolate. Furthermore than that, if we determine the presence of the diphtheria bacillus by a single culture from the throat, we are pretty sure to be led astray. The diphtheria bacillus has a habitat which extends over a considerable surface of the mucous membrane.

I have always been very sorry that the results of the double cultures from the throat and nose which were first undertaken here in Boston were not published. If they were, I have never seen them. I should very much like to see them, to see how frequently the bacillus is found in the nose when it is not found in the throat. We have made a considerable number of examinations in Providence,— some by the State Board of Health, by Dr. Swarts, and some by the City Board of Health; and it has been found that the diphtheria bacillus, certainly among persons who are not sick, is more common in the nose than it is in the throat. I think investigations of others have shown the same thing. I think that the examinations at clinics in Philadelphia showed that a very large proportion of persons who had disease of the nose, who went to the clinic on account of disease of the nose, had diphtheria bacilli present, so that, if we should take cultures from both the throat and the nose, we should find the number of persons who were infected very much greater than we now imagine it is. Furthermore, it is well known that a single culture is not sufficient to determine the presence of the bacillus. It is well known that, if a culture is made to-day, both of the throat and nose, and no bacillus is found, it may be taken to-morrow, and the bacillus be present. It is rarely the custom — though it is so in Boston, New Bedford, and Newton, I think, and perhaps some other Massachusetts cities — to require two negative cultures; but, to be of real value, these should be required from both the throat and the nose. Even if they were thus taken, we should find that a good many which would pass would, if they were examined later, be found to have the diphtheria bacilli present.

Now we cannot attempt to apply such rigorous methods as these.

If we attempt to find diphtheria bacilli by several successive cultures from the different portions of the mucous membrane of all the persons that we can get hold of in any way, we shall find that we have on our hands a very large number of people who are infected with the diphtheria bacillus, and who are to be isolated. It may be said, as the writer of the paper did say, that it is quite likely that a good many of these persons do not have virulent bacilli present; but that does not help the solution of the problem very much. In the first place, I cannot find from reading that the so-called test of virulence upon guinea pigs is entirely reliable: we may get a very virulent colony, and we may not. Furthermore, we are not positive that what is virulent for the guinea pig is virulent for a human being. Furthermore, if we have such a large number of cases, it is manifestly impossible to apply this biological test for the virulence of the bacillus. So it seems to me that the problem of how to deal with the diphtheria bacillus as it is found in human beings at the present time, especially among those who are able to be about, is, as Dr. Diggs has said in regard to the problem of how to deal with well persons who are infected with the typhoid bacillus, an insoluble problem.

The Chairman.— Has Dr. Hill anything to say on this subject?

Dr. Hill.— The only thing I should like to say is that, after all, it is probable that the practical way to meet the present difficulties is to isolate these people carrying diphtheria, so far as you can and when you find them. Dr. Chapin proposes that we should find them all, or attempt to; but that is not immediately possible. The thing is, What are we to do to-day and to-morrow and next week? and it seems to me that certainly those with diphtheria bacilli in their throats, as they are found, should be isolated.

The essay itself is a very excellent one, and represents my own ideas on the subject very well. I should like to say — I have brought this matter up before elsewhere — that it seems to me that the boards of health, if they intend to isolate on the finding of the bacillus, should alter their regulations, so as to make the presence of the bacillus sufficient for isolation. As I understand the thing, at the present time boards of health usually require the isolation of

cases of the disease of diphtheria. You cannot get a man to go on the stand and say that a patient has the disease of diphtheria because he has diphtheria bacilli in his throat, if he is not ill. I would suggest, as a practical proposition before the Association at the present time, that regulations should be so altered as to allow isolation on the finding of the bacillus. There may be a dispute over the diagnosis of the disease, but there can but rarely be a dispute over the presence or absence of the bacillus. If I am not mistaken, Dr. Chapin himself has had in that very line a difficulty which would have been entirely gotten over if he had had such a regulation as that, allowing the isolation on the presence of the bacillus and not on simply the disease of diphtheria.

The Chairman.— Dr. Brough, of Boston.

Dr. Brough.— The only practical point regarding which the presence of the diphtheria bacilli concerns us is chiefly in these convalescing cases. We have them constantly in the city, running over a period of weeks after the disappearance of all the membrane; and, practically, no isolation is carried out. They do not like to take the cases in the hospital, and we cannot release them; and it causes us a very great deal of inconvenience. It leads to a certain extent to a violation of the regulations of the board of health, which require people to isolate these contagious diseases; but it is almost impossible to make these people understand that, when they have the bacilli in their throat, they are contagious. You may talk to them and tell them to isolate them, but they will not isolate them.

As regards the contagiousness of the diphtheria bacillus in the throat, I do not think that it is so very contagious. In the large number of cases we have had in the city where positive cultures have been running, I have only in one case seen membrane or another case become affected from the diphtheria bacillus. All the other cases, notwithstanding the fact that a very large number of children had been exposed to the disease, have never developed any membrane cases. I had almost made up my mind that, after the membrane had disappeared and a certain time had elapsed from the presence of the disease, these diphtheria bacilli in the throat were probably not contagious at all; but I was compelled to change my

mind in that respect, because there was one case that we had in the North End where the throat of the child had cleared up. During the time that it was sick, the sister had been sent away. That child had a positive culture running over about three weeks. The child was running round, and the family thought that it was all right. They allowed the sister to come home. This sister slept in the same bed for two or three days with that child with the positive culture; and it developed a very large amount of membrane, so I was compelled to change my opinion. While I think they are not so very contagious, I think they may be. I should say the question is one that I do not know exactly how it can be settled. The only way I can see that we can do is, where we get positive cultures and where they persist and where people will not isolate them, even if there is no membrane present, to remove them to the hospital as if they were membrane cases. They are to a certain extent contagious, but I do not think they are anywhere nearly so contagious as those cases are with membrane.

The Chairman.— I think Dr. Ripley, of Brockton, has some experience in this line.

Mr. Cary.— Dr. Ripley has gone home. He was better informed in the case than I was. Of course, as the doctor knows, it was a case that got away from the Boston authorities and came to Brockton. The Boston authorities sent some officers after him, and he did not see fit to come back. Dr. Ripley took a culture of the person's throat, which was positive. The young man was about seventeen years old, if I remember right. I have not seen the case. Dr. Ripley had posted himself in the matter, but he had to go away. After the officers came from Boston, this young man went to the police station. They advised him there to go to a physician and get a statement from him: that was before we got hold of it in the Board of Health. He brought the paper in, and showed it. The physician called him a well man, and walked out. But the culture showed what the trouble was,— it was sent to Boston, I think to Dr. Hill,— and three cases developed in the family. What was the time that they developed, that I have no information of, but very quickly. I think that within forty-eight hours from the time the patient from

Boston went into the family the first case developed. That case was reported as membranous croup. About twenty-four hours later another case developed, which was pronounced diphtheria; and about thirty-six hours later, another case of diphtheria. It shows that to all appearance the patient was perfectly well, as far as could be seen. Our city physician pronounced him so. They did not examine him for that particular disease, of course; but he appeared to be perfectly well. But, still, those three cases developed right in that family, probably from that one case. He is in quarantine to-day. As I told Dr. Durgin over the telephone, we hold him there yet, and hope to keep him there until we get some negative cultures. He himself is, to all appearance, perfectly well; but, still, those three different cases came into the family that he went into.

The Chairman.— This was a young fellow who ran away from Boston while we were hunting for him, and the fact that several cases developed in the family into which he went in Brockton is very interesting in this connection. Of course, we had no authority to bring him back or to do anything with him after he escaped from our borders. I think the Board of Health of Brockton is entitled to great credit in its ready care of this young fellow.

Mr. Cary.— There is one other matter that I would state. The second child that came down was a girl, who was taken at school, was sick for a whole half-day in one of our schools. Friday afternoon she was taken. I think they have been through the school every forenoon since, and examined all the scholars in the room,— our city physician and Dr. Ripley have been through, and examined the scholars,— but they have found no farther cases outside of that family as yet.

Dr. Mason.— Mr. President, there is one question that I should like to ask. One of the first of these ten cases cleared up very rapidly, and I got very nearly a negative culture after the child had been in quarantine two weeks or so. A few days later the physician sent in a culture from this same case, and it was almost a pure culture of diphtheria. It was taken from a fresh membrane on one of the tonsils, and it looked to me like a case of reinfection. I should like to inquire if Dr. Hill or any one else present can tell me if that is a common occurrence.

Dr. Hill.— I did not catch the question.

Dr. Mason.— The throat had nearly cleared up, and it started in again fresh.

Dr. Hill.— In about 30 per cent. of our cases (we take two negative cultures for release) we get a negative, and the next day perhaps an almost pure culture of the organism. That is why we require two consecutive negatives for release.

Dr. Mason.— Is that connected with the formation of new membrane?

Dr. Hill.— Oh, no, not usually.

Dr. Mason.— This had a new membrane formed.

Dr. Hill.— Sometimes we get cases where the physician says it is a recurrence, and membrane has come back,— very seldom, though.

The Chairman.— Dr. Denny, will you close the discussion on this subject? I have the honor to introduce Dr. Denny, the author of the paper, who will close the discussion.

Dr. Denny.— Mr. Chairman, I want to thank the Association for the honor it has done me. I should just like to say a word, Mr. Chairman, in regard to what Dr. Chapin has said. As I understood him, he thinks that there are so many of these cases that it is impossible to isolate them; and he does not think that it is practicable to make a distinction between the virulent and the non-virulent cases. It seems to me that that is the important question, whether the bacilli are virulent or not; and I believe that there were very few cases in this community where there are virulent diphtheria bacilli in healthy persons. In the cases which Dr. Chapin referred to, which were reported by Dr. Park,— quite a large series,— the greater number were non-virulent. I think the hygienic conditions under which the patients are living also make a great deal of difference. If people are living under good hygienic conditions, there are very few of these cases. During this last year there were 235 cultures from healthy individuals which were sent to the Brookline Board of Health laboratory. Most of these persons were living under very good hygienic conditions; and out of this number — although most of them had been somewhat exposed — there was only one positive case. Certainly, in a suburban community the number of these cases is very small indeed, except where there is great exposure.

In regard to the value of the guinea-pig tests for the virulence of bacilli, I think that most investigators believe that that is a sufficient test. All the cultures from real cases of diphtheria show the presence of virulent diphtheria bacilli; and, if the bacilli do not produce toxin in bouillon to kill the guinea pig, they are not likely to produce toxin in human beings and produce diphtheria.

In regard to the regulations of boards of health, it seems to me that boards of health should consider as diphtheria all persons who have virulent diphtheria bacilli in their throats. The occurrence of constitutional and local symptoms in the case may be taken as sufficient evidence of virulence. Where these symptoms are not present, the bacilli ought to be tested; and, if the bacilli are virulent in guinea pigs, the case should be considered as true diphtheria, and isolated.

Mr. Stone.— Mr. President, we have — as, possibly, some of you know — in Newton a widely scattered community, composed of several villages. There is one of our villages in which taking cultures has been possibly more general than in any other section; that is, it has appeared to be the practice of the physicians in that particular district of the city to take a culture in practically every case of sore throat coming to their attention. It has been the practice of the board, after examination of those cultures by a bacteriologist, to proceed immediately to isolate a case, or, more generally, send the case to the hospital. The result has been that we have had from that district an unusually large number of diphtheria cases, with an unusually small spread of the disease from the original cases, which points out to us that the solution of the question, under present conditions and at the present stage of the knowledge of the matter and at the present stage of public sentiment, is the willingness of physicians to use the privileges of the bacteriological laboratory; that is, in this section of the city where the physicians have used it very generally, and have very frequently expressed surprise at securing a positive report from a case which they had diagnosed as tonsillitis or as ulcerated sore throat. We have had a very small spread from a comparatively large number of positive returns. In other sections of the city, where the cases of diphtheria and the spread therefrom have been more numerous, there have been fewer cultures taken; and

the public interest seems to have been served very much less. Which brings me back to the original statement, that at present it seems to be simply a question of the placing of adequate facilities for the taking of cultures at the disposal of the physicians, and educating them to the point of using those facilities to the best advantage.

The Chairman.— I should like to give an opportunity at this time to any one who wishes to present an invitation for the October meeting. There is on hand an invitation from Worcester and also from Fitchburg. If there are members present from those two cities, this is a good opportunity for them to present a preference for the October meeting. That of July is already provided for, as usual; but the October meeting may now be spoken for.

Dr. Woodworth.— Mr. Chairman, in behalf of the Fitchburg Board of Health I renew the invitation I extended last year, and which I have been requested to renew by the present members of the board, that the Association meet with them at Fitchburg next October. I trust that the invitation will be accepted.

The Chairman.— The Association receives this invitation from Fitchburg; and it will be in order for it to go directly to the Executive Committee, where it will be considered, and very likely Fitchburg will be the next place of meeting. Is there any other business to come before the Association?

Dr. Utley.— Mr. President, I have a little something to relate to the Association,— an experience that our board had two or three weeks ago. Perhaps some of the other boards have had the same. In one of the sections of our city we had under suspicion that there was some case of diphtheria, notice of which had not reached the board. We did not know just where to locate it, but there were a few cases breaking out here and there in this section. One day, about five o'clock in the afternoon, our board received a communication over the telephone from a physician, saying that he had been called to a very severe case of diphtheria that was *in extremis*. Our agent repaired to this house, and found that the child, which was two years and a half old, had been sick for nearly a week, and had been treated by a Christian Scientist, who told the family that there was nothing

the matter with the child, of course. Here was this family going in and out. There was no quarantine, of course, or anything of the sort. This doctor who was called in said that the child could live but a few hours. In fact, the child died at nine o'clock; and the doctor was called in about four. The question with us was, What can we do with that family, if anything? or, if we have no means of reaching those cases, ought there not to be some legislation whereby we can? That is only a suggestion that I bring before the Association. Perhaps other cities are having the same experience with such cases. But it is quite a problem with us, and we think that it is the source of a number of cases of diphtheria in that particular section of the city.

THE CHAIRMAN.— I see no way under the present law for reaching such a physician or the parents unless you can prove that they did know that it was diphtheria. As I understand from your case, they did not call it diphtheria.

DR. UTLEY.— No, they did not call it anything, I suppose. The child was not sick. It was practically dead when the doctor went in.

THE CHAIRMAN.— I see no way of reaching such cases unless you can show that they knew it was an infectious disease. Is there any other business to come before the meeting?

On motion, the meeting was then adjourned.

# JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH

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## July Meeting, 1900

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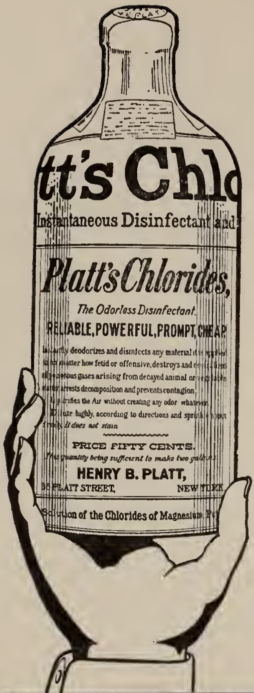
SUBJECTS: Service Pipes for Water Supplies that corrode Lead and other Metals — The Cesspool

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## THE JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH was organized in Boston in March, 1890, with the following objects: the advancement of sanitary science in the Commonwealth of Massachusetts; the promotion of better organization and co-operation in the local Boards of Health; the uniform enforcement of sanitary laws and regulations; and the establishment of pleasant social relations among the members of the Association.

All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

THE OFFICIAL JOURNAL OF THE ASSOCIATION is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them.

All communications to the Association should be addressed to the Secretary, **EDWIN FARNHAM, M.D., City Hall, Cambridge, Mass.**

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VOL. X. October, 1900. No. 3.

## JULY MEETING

OF THE

## Massachusetts Association of Boards of Health.

The July quarterly meeting of the Association was held at Gallup's Island, Boston Harbor, on the afternoon of July 19, 1900, the President, Dr. H. P. Walcott, in the chair. The papers presented to the meeting and the discussion thereon follow: —

## SERVICE PIPES FOR WATER SUPPLIES WHICH CORRODE LEAD AND OTHER METALS.

BY R. S. WESTON.

The so-called "lead-pipe question," though not a new subject for discussion, has reappeared with a renewed interest in this State, partly because of the increased care with which matters relating to the public health are being studied and partly on account of the recent publication by the State Board of Health of a report upon certain wide-spread cases of plumbism of a serious nature.

Lead-poisoning is, of course, a familiar subject to all of you; and it is not my intention to discuss either the toxicity of water containing small amounts of lead or the maximum amount of lead which

can be safely allowed in a water supply, with due regard to the public health, but rather to call your attention to the various kinds of service pipes in common use, and discuss with you their comparative values from a sanitary and chemical standpoint. The discussion from the hygienic standpoint was ably led by Dr. Abbott at the last meeting.

The chief kinds of service pipes and their present comparative cost for the 1-inch size are tabulated in the table below: —

| | Price per linear foot. |
|---|---|
| Plain black wrought iron | $0.08 |
| Tarred " " | .09 |
| Cement-lined " " | .11 |
| Galvanized " " | .11 |
| Lead-lined " " | .225 |
| Tin-lined " " | .48 |
| Plain lead (Boston weight) | .36 |
| Tin-lined lead | .72 |
| Brass | .36 |
| Wood (1½ in.) | .11 |

Of these kinds of pipe, the plain, tarred, galvanized, and cement-lined iron and the plain lead pipe are most commonly used. Lead-lined iron pipe has been supplanting lead pipe of late, but the cost of tin-lined pipe is generally prohibitive. The same may be said of the brass or composition pipe, except for inside house connections.

From a sanitary standpoint, all pipes may be classed according to the character of the surface in contact with the water. All of these surfaces are of metal or wood, or metal protected by some coating or lining. The exposed metal surfaces are iron, zinc, lead, tin, copper, and brass.

At this point it may be stated, as a general rule, that the waters which dissolve one metal will dissolve any other commonly used metal. Chemically pure water, simply $H_2O$, does not appreciably attack any of the metals commonly used for services. However, the presence of oxygen or carbon dioxide in an otherwise pure water makes it more or less of a corrosive agent. Hence most natural waters have some action on metals.

These gases in solution, singly or combined, are the two principal factors in ordinary waters which effect the solution of exposed metal surfaces in service pipes.

I will take up in detail each of the varieties of pipe in the table, and have here samples of each for your inspection.

*Wrought-iron Pipe.*— Wrought-iron pipe is the cheapest of all services and the one most generally used. Its advantages are, among others, its low cost, its convenience, and its non-poisonous nature, when dissolved in drinking water. Its disadvantages, however, are many. It rapidly rusts and corrodes, and often fills up solidly with rust and scale after a short period of service. Therefore, it is neither durable nor economical. The water from such services stains closets, bowls, and sinks, converts the universal beverage — tea — into ink, and often plays havoc with the family washing.

*Galvanized Zinc-plated Pipe.*— Zinc is a slight protection only on account of its solubility in waters which attack iron and other metals. The zinc dissolves from the iron pipe in such waters, leaving, of course, the iron surface exposed to the water with all the disadvantages of the latter metal. Zinc, however, is not a cumulative poison in amounts liable to exist in water drawn from galvanized iron services. Galvanizing protects the outside of the service against the action of the soil to a great extent.

*Lead.*— Lead has been used for conveying water since ancient times. Ancient cisterns were lined with lead, and the Roman aqueducts were made water-tight with the same metal. Cases of lead-poisoning were noted during the Middle Ages.

There are many reasons for using lead pipe for services. It is pliable and easily worked. Therefore, the settling of water mains, sewers, and soil, does not cause it to break, neither is it necessary to be so careful about the trenching and laying as when stiff iron pipe is used. It is very durable in most cases, and does not fill up with rust, neither does it scale off in distinguishable flakes.

On the other hand, lead pipe is expensive to buy and to work, and, as is well known, is soluble in certain waters, especially in the soft-ground waters of New England. This solubility is, of course, the primary cause of lead-poisoning. This solution is practically prevented in certain waters, notably in the hard, clayey waters of the West by substances already in solution, such as silica, which retard the action of the water, and by substances in solution and suspension, such as carbonates and clay, which ultimately cause the forma-

tion of protective coatings on the inside of the pipe, practically preventing further corrosion or solution. The chief constituent of these protective coatings is a basic carbonate of lead, the exact formula of which, however, is not known. This basic carbonate acts as a matrix in some cases, binding some of the suspended matter into a coating. Lead pipe is quite satisfactory where the character of the water is such that solution is practically nil. Lead-lined iron pipe has most of the advantages and disadvantages of plain lead pipe. It costs less, however.

*Tin-lined Lead and Tin-lined Iron Pipe.*— Tin is the least soluble and, next to iron, the least toxic and the most satisfactory of all the metals usually exposed to water in service pipes; but its high cost is against its general use. No protective coatings form on tin, and the metal is quite rapidly dissolved in certain waters. This solution may be aided by galvanic action caused by impurities in the tin itself; but pure tin is quite soluble in water containing free carbon dioxide and dissolved oxygen. Although the tin thus dissolved is not dangerous to the public health, the lead or iron pipe underneath may be laid bare, with the consequent evils resulting previously referred to.

*Brass and Copper Pipes.*— Brass and copper pipes are used for hot water pipes and for other house connections. Waters which attack lead, iron, and tin, may also attack copper and brass. Cases of copper-poisoning are rare, however; and the use of this metal where convenience and appearance demand it is allowable.

The characteristics of metal pipes are tabulated below.

(Figures denote the relative degrees.)

| | *Solubility.* | *Toxicity.* | *Durability.* | *Cost.* |
|---|---|---|---|---|
| Plain iron . . . . . . . . . . . | 1 | 5 | 6 | 1 |
| Galvanized iron . . . . . . . . | 2 | 2 | 5 | 2 |
| Lead . . . . . . . . . . . . . . | 3 | 1 | 3 | 4 |
| Lead-lined iron . . . . . . . . | 3 | 1 | 4 | 3 |
| Tin-lined iron . . . . . . . . . | 4 | 4 | 2 | 6 |
| Copper (brass) . . . . . . . . . | 5 | 3 | 1 | 5 |

The best pipe to use in any particular case must be determined largely by local conditions. No general rule can be laid down. Often an investigation is necessary. If upon investigation the ex-

pert finds that a water supply dissolves lead service pipes or that cases of lead-poisoning occur or are liable to occur, what remedy is there?

Obviously, lead pipe services are out of the question; brass and tin lined pipe, at present prices, are generally prohibited by their cost; galvanized pipe is too short-lived to be economical; and plain iron cannot be used advantageously on account of corrosion and iron rust. Therefore, the logical thing to do is either to use wooden pipes or to protect iron pipes with some durable and non-disintegrating coating.

*Wooden Pipes.*— Wooden pipes have been used for centuries, but until lately they have not been made to withstand pressures of over fifteen to twenty-five pounds. The manufacture of wooden pipes has developed with hydraulic mining; and they are now being used extensively in the West for water services, one company alone having orders for over fifty miles for the season's delivery. These wooden pipes withstand pressure up to two hundred pounds. They are durable, and do not impart any odor or taste to the water. They are not made smaller than 1.5-inch internal diameter, which size costs less, however, than galvanized iron 1 inch in diameter.

Wooden pipes cannot be so easily jointed and prepared as metal pipe. They cannot be used, therefore, in confined places, and are undesirable in exposed work on account of their unavoidable clumsiness. Nevertheless, they have their uses.

Wrought-iron pipe is a very convenient pipe to work; and, if this can be protected by suitable economical coatings, it would be the best pipe for all-round use. The outside of wrought-iron pipe can be practically protected by asphaltic or japan coatings, but such coatings cannot be readily applied to the insides of pipes and couplings.

Several lining materials have been used for wrought-iron pipe. Among them, enamel, tar, and cement are in common use. Enamel and tar linings disintegrate and scale off, and some of them impart a taste to the water. They have not been considered effective and economical for these reasons. Pipe lined with natural cements have now been in use for some decades, and, as the methods of lining are becoming better understood, are more in favor than ever.

Cement-lined pipes were not used for a long time because of the difficulty of lining the joints. This difficulty, with the attendant rusting at the joints, is now overcome by inserting composition thimbles, which protect the couplings and other fittings against the corrosive tendencies of the water, if they are skilfully put in. Tin-lined iron couplings are being used at Taunton with success.

The success of cement-lined pipe depends upon the care with which the lining is performed.

Cement-lined pipe is not usually sold by the dealers, but is lined by the user. The best lining is effected by giving the pipe one coat of cement, allowing it to partially set, and then finishing with a thin coating of cement grout. This last coating fills up any pores in the first coating which may have been left, and causes the inside of the pipe to have a smooth surface. Coatings should be made of light-gravity, neat cement. The cement should be sifted before use.

Cement-lined pipes can be bent but slightly, and are necessarily larger externally for a given discharge than iron or lead pipe; but, when well lined, they make very satisfactory water services, and retain their carrying capacity indefinitely, being free from tuberculation or other incrustation. Brookline has been using cement-lined pipes for over twenty-seven years with uniform success; and Taunton, Waltham, and many other municipalities have done likewise. The outside of the cement-lined pipes should be covered with an asphalt or tar coating, to protect them against the action of the soil.

In conclusion, we may therefore say: —

All waters attack metals more or less, the amount of action depending upon their chemical composition.

Corrosive waters should not be conveyed in unprotected metal (except tin) pipes, especially not in lead pipes.

The only practical substitute for metal is wood, and even this is inferior to cement-lined pipe because of its clumsiness and large size.

Cement-lined pipe, if well made, will last a very long time; and, now that means of protecting the joints have been perfected, it seems to be the best service pipe to use with metal-solvent waters, especially where tin-lined iron is too expensive.

Dr. Bennett F. Davenport.— I am very much interested in this subject, as I have been for several years chairman of the Water Board as well as of the Board of Health of Watertown. We have had experience with all the usual kinds of pipe; but we have used the cement-lined iron pipe in our newer work, and consider it the best there is. I have had unusual personal experience with tin-lined lead pipe. Inspection of some of the pipe made by one of our most reputable companies disclosed the fact that the interior surface of the pipe — the tinned surface — was all crackled, and, after a year's use, innumerable pits had formed between the crackles, apparently erosions through to the lead surface. I examined the water, but could find no trace of lead in it. The occasion of these erosions I could not determine. Probably it was galvanic action. In bending up and unbending the pipe, the harder tin lining had been cracked through probably. I could not determine why there was no lead in the water.

Dr. J. E. Sanborn, Melrose.— We are all familiar with the action of carbon dioxide upon lead pipes; that it forms a carbonate of lead which, coating the inner surface of the pipe with its insoluble covering, has been and may be regarded as a natural protection. But I rise to ask if this may be considered a permanent protection.

Does not this carbonate scale off, exposing a fresh surface of lead to poison the water? and may not this scaling process, with its attendant slow poisoning, continue indefinitely? If so, can we ever feel quite safe in trusting lead pipe, no matter how many years in use nor how thoroughly carbonated inside? I beg to ask for information.

R. S. Weston, Boston.— There is some danger from some waters, especially those containing but little organic matter to act as a binder of the carbonate. I had a case last fall of this kind. The water was one which had a minimum action on lead, and the sample was taken from a long line of pipe which had not been used for some time. On flushing it, a large amount of carbonate of lead was carried out, so much that the water was plainly turbid therewith. This dislodgement of the protective coating is the great objection to the use of lead pipe. Suspended lead, when taken into the stomach, acts, of course, the same as lead in solution.

B. F. Davenport.— Galvanized iron is iron coated with zinc,

which commercially nearly always contains some lead. In my experience, I have known of an instance where some 200 to 300 feet of galvanized pipe discharged water into a wooden reservoir, the bottom of which became covered with a white sediment. This I found on investigation to be composed almost wholly of lead. I am accustomed to tell people, who ask my advice about buying galvanized iron pipe, that water will surely in time dissolve off the zinc, leaving a pipe inferior to what they would have had, had they bought plain iron pipe in the first place. They have thus the privilege, if they desire, of paying an increased price for an inferior iron pipe.

Professor L. P. Kinnicutt, Worcester.— Is the so-called rustless iron pipe used any? There is or was considerable use of it in Germany. It is prepared by heating the pipes to redness, and passing steam through them, which creates a magnetic oxide. What action does water have on such pipes?

Mr. Weston.— I have known of rustless iron pipes being discussed by those interested in such matters, and of efforts being made to get at information on the subject. But they are not used in this country, at least to any great extent. I have been unable to find any instance of their use. This coating, it seems to me, must be attacked sooner or later, and must be reduced or oxidized. Their use has been generally given up abroad.

Dr. Gage, Lowell.— Is the amount of carbonic dioxide in water a measure of its liability to attack lead pipe?

Mr. Weston.— Other things being equal, yes. The oxygen present will attack the lead, regardless of the carbon dioxide.

F. E. Forbes, Brookline.— For twenty-five years I have given much study to the problem of service pipe,— to find something which will stand the corrosive action of water and yet be a perfectly safe pipe to use, from a sanitary point of view, under all conditions. I have come to the conclusion that cement-lined wrought-iron pipe, with couplings protected with composition ferules, is the best and safest kind of service pipe.

Some waters do not attack lead pipes as badly as others; but we do not know when changes, or what changes, may come to any water supply. Hence we want a pipe which will be safe under all conditions which may arise.

Experience has convinced me that iron pipe can be lined with cement in such a way that the pipe will not rust up, and the water drawn in the houses through it will be of the same quality as that in the mains outside. Such pipe is absolutely safe, cheap, and will last from thirty to fifty years under ordinary conditions.

MR. HILL.— How can private individuals wanting only a few feet of service pipe get cement-lined iron pipe? It is not on the market.

MR. R. S. WESTON.— I would correspond with Mr. Forbes and other water-works superintendents who have had experience with this kind of pipe. They may be able to furnish you with, or tell you where you can get, some. It is not on the general market. If you cannot get cement-lined pipe, I think tin-lined is the next best thing, though wood pipe can be used in some cases. Wood pipe is very useful to convey spring waters for long distances, and has been used on a number of country estates for this purpose.

F. A. WILLARD, Lancaster.— We get cement-lined iron pipe by buying it of the town. Clinton furnishes us with water, and puts in the pipe. We can buy it of that town at cost.

MR. FORBES.— We furnish cement-lined iron pipe in small quantities to any one in Brookline and vicinity for their own grounds.

## THE CESSPOOL.

BY LEMUEL P. KINNICUTT, OF WORCESTER POLYTECHNIC INSTITUTE.

*Mr. President and Gentlemen,*— About ten years ago I had the pleasure of listening to an address given before this society, in which nearly all the ills which humanity is liable to were traced back to the cesspool. The address represented the general opinion held at that time; yet to-day the latest work on sewage disposal shows us that our forefathers, in using the cesspool, were using, unawares, a most efficient method for the disposal of solid putrefying substances. These rapid changes of opinion in sanitary science regarding every-day matters are most confusing; for what we hold as true to-day, we are told to-morrow is all false. We all of us remember the numbers of meetings when plumbers' laws and regulations have been discussed,

and the dire effects that must follow the breathing of sewer gas; yet men like Abbott, of Pennsylvania, and our own Vice-President now tell us that no distinct disease can be traced to sewer gas.

The Massachusetts State Board of Health in 1887 investigated the subject of ice; and the belief was that in large cities like New York, or in any place where ice was taken from an unknown source, ice-water should be prohibited, as there was great danger of contracting typhoid fever in this way.

So with the cesspool. As I have said, ten years ago we regarded it as a most unholy and obnoxious survival of the unsanitary life of our forefathers. Now we are taught, not only that it was not, after all, a very bad method for the disposal of household refuse, but are shown the advisability of using it on a large scale for the partial purification of the sewage of cities.

What has caused this change of opinion regarding the cesspool? It is due chiefly to the careful consideration of a fact well known to us all; *i.e.*, that, although a large amount of solid animal and vegetable matter was being run, day by day, into the old-fashioned cesspool, the cesspool did not fill up, and at the end of the year the amount of solid matter at the bottom was very small in comparison to the amount that had been added during the year. Do not most of us remember the cesspool of our boyhood days that was not cleaned out oftener than once in three or four years? Yet how few of us ever asked the reason why it was not necessary to do so every three or four months.

Why this was not necessary and how the cesspool has been adapted to the disposing of sewage on a large scale, I take as the subject of my fifteen-minute talk.

To answer the first part of this question, it is necessary to speak briefly regarding bactéria and the work they do in decomposing dead animal and vegetable matter.

Bacteria are a low form of vegetable organism, which multiply by a process of transverse division. Though commonly considered as dangerous enemies, they are, in fact, our greatest friends; for it is only a comparatively few species, those that exist on living matter, that cause disease. The great majority obtain their nutritive material from dead animal and vegetable matter, and resolve these substances

into simpler compounds, as carbon dioxide, water, ammonia, nitrates, which are the food supply of higher vegetable life. Were it not for the presence of these bacteria, disposing, as they do, of effete organic matter and changing it into plant food, human life on this earth would, seemingly, be impossible. These are the bacteria that are present by the hundreds in a cubic foot of air, and in a cubic centimetre of so-called pure water, and by the millions in a cubic centimetre of polluted water, or of sewage.

Bacteriology as a science may be said to date from Pasteur's experiments on the causation of the putrefaction of beer and the souring of wine, in the middle of the present century; and it is only at a very recent date that there was any reliable knowledge as to the action of bacteria in the purification of sewage.

Fresh sewage contains, in immense numbers, the class of bacteria which live on dead organic matter, and which cause its decomposition. These bacteria can be roughly divided into two great groups, each containing numerous species. These groups are called the anaërobic and the aërobic groups. The anaërobic group embraces all those species that live, grow, and multiply out of contact with air and light; the aërobic, those species that live, grow, and multiply best in air and light. Each group plays its own special part in the destruction of effete matter and the organic substances contained in household waste. The anaërobic bacteria act first. They disintegrate the solid animal and vegetable matters, liquefy them, and bring them into solution. The aërobic bacteria act upon these disintegrated and liquefied compounds, and by a process of oxidation change them into harmless gases.

For the destruction of dead organic matter, both groups of bacteria are necessary,— the anaërobic to disintegrate and liquefy the complex organic substances, the aërobic to change those simplified and liquefied compounds into harmless products.

Taking the above as a brief outline of what takes place in the decomposition of excreted and effete matter, we are in a position to consider the changes that take place in the cesspool.

Into the cesspool enter all the waste products of human life, and with these waste products millions of bacteria, both anaërobic and aërobic. But, the contents of the cesspool being practically shut off

from air and light, it is a most suitable environment for the development of the anaërobic bacteria. These bacteria immediately begin to act upon the solid compounds in the excreted and other household waste. The complex nitrogenous substances are broken up and liquefied, also compounds like cellulose in its various forms, as paper, starch, woody fibre, are disintegrated; and, finally, if the action continues a sufficient time, all that remains in a solid state is practically what we can call the ash, or the mineral matter of the animal and vegetable substances that entered the cesspool. The action that takes place in the cesspool might be compared to the process of combustion. If the substances that entered the cesspool had been burnt in a furnace, we should have left behind only the ash, all else being given off in the form of various gases. In the cesspool the bacteria have taken the place of heat. All the solid matter that remains is the ash. All else, however, has not been changed into gas, but, to a great extent, into substances soluble in water.

Liquid that passes from the cesspool as it leaches through the soil, or as carried away by a drain and allowed to run on land, comes in contact with air; and the conditions become favorable for the development of the aërobic bacteria, and the final process of purification begins. The liquefied compounds are most easily acted upon by the bacteria, and the compounds changed, as I have said, into harmless gases and mineral substances.

It thus appears that our forefathers, without any knowledge of bacteriology, devised one of the most efficient methods for the purification of household waste, and one which I will show in a moment has, during the past two years, been utilized for the purification of sewage in large cities, and has great promise of being most successful.

The question as to the dangers attached to a cesspool I do not at this time care to consider at any length; but it seems to me that the danger arising from the use of cesspools has been overestimated. Certainly, in the decomposition of the solid complex compounds by the anaërobic bacteria, a large amount of gas is given off. This gas, however, is composed chiefly, as I will show later, of carbon dioxide, marsh gas, hydrogen, and nitrogen, and resembles concentrated sewer gas, but does not contain, as far as we know, any amount of sulphite of hydrogen or other poisonous gas.

If the liquid in the cesspool is on different levels at different times, so that the sides at times become dry, there may be danger of some pathogenic germs being in the gas that escapes from the cesspool; but the above condition cannot exist if the inlet pipe enters the cesspool at a lower level than the exit drain.

If the liquid in the cesspool percolates through the sides of the cesspool and the soil is hard clay, it may, while still containing a large amount of soluble polluting matter, enter a water supply, as a well or a stream; but, if the cesspool is in sandy soil, there is very slight, if any, danger, as late experiments have shown that organic matter in the condition that it is in on leaving the cesspool is changed with the greatest rapidity into absolutely harmless compounds. If the liquid is carried by a pipe from the cesspool and discharged over land not near a well or stream, it seems that there could be no danger. As to the escape of any dangerous gas from the soil near a cesspool, I do not see how it is possible.

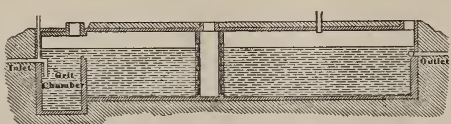
Having thus tried to answer the question why cesspools do not quickly fill up with solid matter, and also to describe what changes take place in the cesspool, I wish, in the few minutes at my disposal, to describe how this method has been made use of by sanitary engineers for treating sewage on a large scale.

Mr. Scott-Moncrieff, the well-known sanitary engineer of Ashland, England, was, I believe, the first to recognize that the bacterial purification of sewage took place in two stages, and that the first stage should serve as a prelude to further treatment. He erected in 1891 at Ashland, a tank, or cesspool, for the express purpose of liquefying the solids in sewage, in which the sewage was forced upward over a bed of stones. (The diagram on p. 86 represents his liquefying tank.)

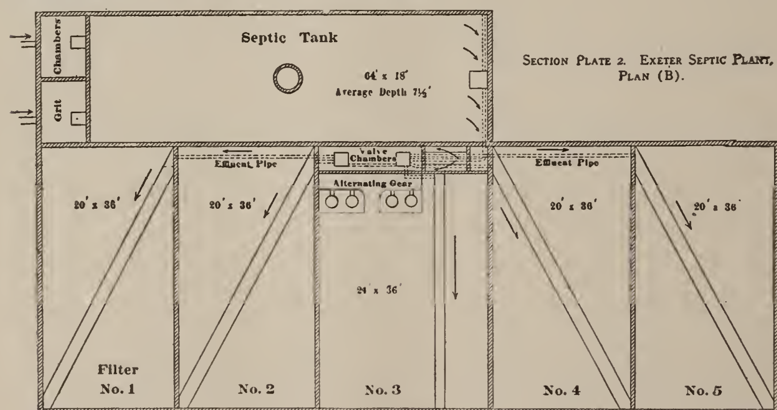
The credit, however, of showing that a modified cesspool could be advantageously used for the treatment of sewage of cities and towns belongs to Mr. Cameron, of Exeter, England. The process was named by him the septic treatment, and it is by this name that it is now known.

He built at Exeter, England, an underground tank of cement concrete, sixty-five feet long, sixteen feet wide, and of an average depth of seven feet, having a cubical capacity of 53,000 gallons. The tank was covered with a concrete arch; and a portion of the tank near the

inlet was made about three feet deeper than the rest, and partially cut off by a low wall, forming a couple of pockets, or grit chambers, to retain sand, grit, and road washings. The inlet was carried down to a depth of five feet below the surface, so that air could not make its way down with the sewage, and also so that gases could not escape from the tank back into the sewer. The effluent outlet was also below the level of the liquid; and, to avoid any current that would be liable to carry with it any of the floating matter from the surface, a cast-iron pipe was carried across the whole width of the tank fifteen




Exeter Septic Tank. Section (A).




Section Plate 2. Exeter Septic Plant, Plan (B).

inches below the surface, and on the lower side of this pipe there was a continuous opening about one-half inch wide. An iron pipe about one and one-half inches in diameter extended out of the top of the tank to allow the escape of gases which were formed, and the sewage in the tank could be inspected by opening a manhole at one end or descending into a manhole constructed in the centre and having glass windows.

At the plant at Exeter there are also five beds (Plate 2 B), thirty-six feet long, twenty feet wide, and four feet deep, made water-tight and filled with crushed clinker or coke breeze. Such beds are admi-

rably adapted for the cultivation of aërobic bacteria; and the liquid as it comes from the tank, having undergone the first stage in the process of purification, is now in just the right condition to be acted upon by the aërobic bacteria.

In August, 1896, the main sewer of St. Leonard's, a suburb of Exeter with a population of 1,500 and an average daily flow of sewage of 57,000 gallons, was connected with the tank; and the above volume of sewage has been continuously passed through the tank since that time. The effluent from the tank falls over a wall about one foot high, which effects slight aëration, and then by means of the alternating gearing is caused to pass automatically on one or the other of the beds.

Visiting the plant, the effluent from the tank first strikes the attention. It is a brownish-colored fluid, free from palpable suspended matter, but does contain solid matter in a state of very fine subdivision, almost like river silt. It has an odor, but at the four different places where I examined it the odor is not more offensive than that given off from the first or second basin used in the chemical precipitation process.

The liquid in the tank is covered with a thick, floating mass, dark brownish-yellow in color, through which bubbles of gas are continually breaking. This floating mass is said to have formed during the first six or eight weeks the tank was in operation, and not to have increased much in thickness since that time. It appears to be composed of organic matter, but, in fact, I believe it is principally a mass of micro-organisms; and it is not till this floating mass has been formed that active septic action takes place. At the bottom of the tank there was last summer a deposit two or three feet in thickness, the accumulation of three years, as the tank has never been cleaned out. This deposit looks like sewage sludge, though it contains much less organic matter. From the iron pipe at the top of the tank issues a volume of gas, which, when ignited, burns with a colorless flame about one foot in height. The unignited gas has no very marked odor, and does not contain appreciable amounts of sulphide of hydrogen.

What are the changes that take place in this tank? They are exactly those that take place in the cesspool. The most marked

change is the disappearance of solid matter, and it has been demonstrated that from one-half to two-thirds of the total solid matter has decomposed and liquefied.

In this decomposition a large amount of solid matter is changed into gaseous compounds, as ammonia, carbon dioxide, marsh gas, hydrogen, and nitrogen. Of these, ammonia and part of the carbon dioxide remain in solution; while the others, being insoluble in water, escape from the tank.

That a large volume of gas is given off from the septic tank is seen not only at Exeter, but everywhere the septic tank is in use. The gas at Exeter burns with a flame over one foot in height. The only determination as to the amount of gas that is given off is, so far as I can learn, one that was made by H. W. Clark, chemist for the Massachusetts State Board of Health, working with a very small experimental tank. His results indicate that from .5 to .6 of a cubic foot of gas are formed from 100 gallons of sewage.

Analyses of the gas have, however, been frequently made; and, though of course it must vary greatly from time to time, the following analyses made by Dr. S. Rideal give an idea of its composition:

| | |
|---|---|
| Carbon dioxide | .3 per cent. |
| Marsh gas | 20.3 " " |
| Hydrogen | 18.2 " " |
| Nitrogen | 61.2 " " |

Who first saw that a similar action to that which takes place in a closed tank must also take place in a large open tank is not known. Undoubtedly, the same thought occurred to many persons at about the same time; for of course the conditions in both must be practically the same.

No free oxygen can be present in an open tank when full of sewage; and, the sewage containing, as it does, a large amount of suspended matter, very little light can penetrate down from the surface. Whoever may have first used an open tank for septic action, I found them last summer being used experimentally at Accrington, Huddersfield, Leeds, and Manchester. In all these places an old tank, which had formerly been used for chemical precipitation, had been turned into a septic tank, all the change necessary being to place the outlet

so that the effluent opening should be three to four inches below the surface of the liquid, although, of course, in the building of such a tank, it would be better to have the inlet also beneath the surface. In all these places the general appearance of the liquid in the tank was the same, very dark and opaque, the surface coated over with a thick layer of solid matter, and the activity of the action in the tank shown by the thousands of bubbles of gas escaping through this layer of solid matter. The effluent was of a dark brown color, containing suspended matter in a very fine state of subdivision; but neither the odor given off from the tank or from the effluent was so offensive as to prevent its use by towns or cities. That the action which takes place in the open septic tank is very similar to that which takes place in the closed tank is shown by a series of experiments made by Mr. Fowler at Manchester. He made daily analyses for one month of the effluent from an open tank and of the effluent from a closed tank, both being supplied with the same sewage. The following table gives the average of these daily analyses. The results, as seen, are almost identical: —

| *Parts in 100,000.* | *Open tank.* | *Closed tank.* |
|---|---|---|
| Free ammonia | 3.20 | 3.10 |
| Albuminoid ammonia | .50 | .51 |
| Oxygen consumed | 8.46 | 8.43 |
| Chlorine | 16.40 | 16.10 |

The real practical value of the septic tank, or we might say the cesspool, is that it destroys suspended matter without forming any great amount of sludge, or precipitate, thus having a great advantage over any chemical precipitation process; that it seems to bring cellulose into at least partial solution, thus preventing the coating over of bacteria beds, either those of intermittent filtration or contact with a layer more or less impervious to water; that it breaks up the more complex organic compounds, forming substances that are more easily acted upon by the nitrifying bacteria than the compounds in raw sewage. There is also another advantage in the use of the septic tank that it seems to me may be of great importance. In all bacterial treatment of sewage the changes that we have followed in the septic tank take place; but in the systems of intermittent

filtration beds and contact beds, the second phase of purification, nitrification, is also being carried on at the same time. If in either of these systems too little air is supplied, the aërobic or nitrifying bacteria cease to work, their place being taken by the anaërobic bacteria, which are unable of themselves to finish the process; and, whenever the anaërobic bacteria have obtained the upper hand, as is the case when a bacteria bed is overworked, the bed becomes foul, and the product obtained is similar to a septic tank effluent. By using a septic tank, allowing the effluent to come in contact with air and then passing it onto a bacteria bed, the two phases of purification are kept separate; and better work is naturally to be expected.

I stated at the beginning of this paper that what we hold as true to-day we are told to-morrow is all false; and so, in a year or two from now, I may come before you, and say that the changes that take place in the cesspool are not due to bacteria, but are due to chemical action. And I say this now because there are certain facts that point in this direction. There is a class of chemical substances known as enzymes, or unorganized chemical ferments, which are the products of vegetable and animal life, and which in minimum amounts, without being themselves used up, are able to break up and decompose large amounts of complicated and insoluble compounds. Thus the enzyme known as diastase decomposes starch; lifase, many fats; cytase decomposes and dissolves cellulose. These enzymes seem to decompose the complex organic substances by what, chemically speaking, is called hydrolysis,— the breaking up of a compound by the addition of water. In this way, substances like albumen may be broken up, with the formation of nitrogen, ammonia, hydrogen, marsh gas, and carbon dioxide; substances like cellulose, into hydrogen, marsh gas, and carbon dioxide. Many of the enzymes being the products of anaërobic bacteria, they would naturally occur in the septic tank; and a large amount of the decomposition may thus be chemical rather than bacterial.

Whatever is the cause of the changes that take place in the cesspool, or septic tank, the changes are those that I have described; and I only hope that I have been able to point out to you that the most modern method of treating sewage is, in fact, the method of our forefathers.

Robert Spurr Weston.— This is a very interesting and important subject. It is particularly interesting to note the enormous strides which the subject of sewage disposal has made in a decade, especially with reference to the amount of sewage which can be disposed of on a given area of bed.

In the eighties we were talking of vast farms as necessary for the disposal of this matter. In the nineties we have been talking of reduced areas or bacterial beds. Since 1896 we have been still further reducing the area, as Professor Kinnicutt has told you, by first putting the sewage through a cesspool, thereby disposing of a large part of the suspended solids of the sewage. Now we are learning how we can still further reduce the areas by using very coarse, porous material, which will more effectually expose the sewage in thin layers to the action of the air and bacteria.

Dr. Gage.— Is there any danger of the pathogenic bacteria which may be in the sewage getting out in the final effluent?

Dr. Kinnicutt.— That depends on the rate of filtration, also the degree of purification. No definite experiments have been made on this subject.

Dr. Gage.— The presentation of the ice question is new to me. I had always supposed the danger of ice from polluted sources was much greater than has been represented. Early in the spring I had a case of typhoid fever breaking out soon after the family had taken a new supply of ice cut from a known polluted source. It had been put in a silver ice-pitcher, from which the person drank freely. I corresponded with Dr. Abbott, and he thought there was some danger from such a source, Professor Sedgwick also testified in court that there was some danger from a polluted source.

Dr. Kinnicutt.— The vitality of typhoid fever germs is greatly reduced by freezing. I did not mean to say that there is *no* danger from ice from polluted ponds.

Dr. Abbott.— Personally, I do not claim to know about the danger from ice. I only take the opinions of those who I think do know. For myself, I do not care to take ice from a source that is polluted. I have recently made a change, in order to get my ice supply from a source that was less suspicious. On the question of cesspools I do not claim to be an expert, but I give you a bit of my experience. I

built a new cesspool fifteen years ago, and it has not been cleaned out yet. It is built of stone, not water-tight; and the overflow runs off through a loose pipe into the garden. My neighbors don't even know that it exists. As it is no nuisance, I don't propose having it made one by having it cleaned out. Whenever the system of sewers is introduced in the town where I live, I shall be glad to take advantage of the system, since that is the better method of disposal.

Dr. Durgin.— It is of importance that the cesspool matter be so dealt with as not to make mischief,— that it shall be handled so that neighbors shall not complain of its being a nuisance. The common cesspool overflows, and perhaps runs over on to the neighbor's premises. As usually managed, it stinks badly, and complaint is made.

Now how shall we tell people to build cesspools so that this shall not happen? If the material is filtered through the proper medium, the effluent may flow into a brook or over the surface, and cause no nuisance; but the trouble in many cases is the want of proper location for the cesspool, with the proper amount of sand or gravelly soil for the necessary filtration.

People come to our office, and say their conditions are such that they must drain their cesspools into the brook or garden. If they locate their cesspools so that there are six or seven feet of sand through which this water may filter, it will not come to the surface to make a nuisance. I am not surprised at Dr. Abbott's case, owing to the fact that the sands of Wakefield furnish sufficient filtration; but we do not all live where there is a sufficient amount of sand for such a purpose. This is one of the most interesting subjects we have had before our Association for years.

James Kimball, Springfield.— Our experience in Springfield corresponds very largely with Dr. Durgin's, although our soil is of sandy formation. The cesspools fill up quickly, and we are troubled by their overflowing. These places are mostly on new streets where there are no sewers, but where there is city water, which at certain seasons of the year contains a large amount of vegetable matter that evidently assists in the formation of a sediment, preventing the sand from absorbing the drainage. Consequently, it must come to the surface. As soon as this occurs, complaints come from tenants and neighbors.

Dr. Kinnicutt.—A cesspool on a hard clay soil is a hard question to deal with. Still, it can be done. It is only a question of dollars and cents. Scott-Montcrieff tank, as this diagram shows, is only a modified cesspool, the liquid contents being allowed to pass successively through nine trays filled with sand. Analysis of the liquid after passing through the last tray shows it to be in such a state that it cannot possibly cause any trouble. For large country places a system of bacterial beds can easily be constructed. In England I have seen such places, where the effluent from the bacterial beds entered an artificially constructed pond, which was filled with fish and surrounded with flowers and shrubs, forming a picturesque bit of scenery.

James C. Coffey, Worcester.— Do you recommend individual cesspools in ordinary city lots ?

Professor Kinnicutt.— Certainly not. In a city with a sewerage system, householders on streets provided with sewers should be obliged to enter them. Cesspools are only for houses which cannot discharge the household waste into a sewer.

Mr. Coffey.—As a practical health officer, I have some knowledge of cesspools. We all know that they are constant sources of trouble. In my city (Worcester), houses are being built very rapidly in new localities, often in advance of sewers. Sometimes a cesspool will run over in a month ; and the neighbors will come into the office in a state of great excitement, demanding the abatement of the nuisance at once. I was in hopes, while listening to the first part of the professor's paper, that I could say to such people : Don't get excited. Have patience. In a few days the aërobic bacteria will get in their work, and there will be no more trouble. But, before the paper was finished, I saw that the system is to be used by communities rather than by individuals. Even in sandy and gravelly soils the cesspools do fill and run over very frequently, although they last much longer, to be sure, than in clay soils. It is my experience that trouble is likely to occur in any soil after a few years. After cleaning out these cesspools, I have advised driving an iron bar down through the clogged soil, in order to get contact with clean soil. After all, in my opinion, these are makeshifts, to be used only until a sewer can be obtained.

Dr. B. F. Davenport.— When you have a sewerage system, what do you do with the cesspools ?

Mr. Coffey.— Clean them out, and fill them up.

Dr. Walcott.— I would say, in behalf of the State Board of Health, that on a legislative order we investigated the ice supply of Massachusetts, examining many samples from contaminated sources. Reports of this investigation and our recommendations appear in one of our annual reports. The result of the investigation was that we recommended that ice from infected ponds should never be used. Investigation in the laboratory has confirmed this, and I see no reason to change my opinion. Let ice from infected ponds alone.

Walter C. Kite, M.D., Milton.— While this little matter of a complaint book is of no interest to city boards of health, it may prove helpful in towns where there are not a large number of complaints, and where the complaints are sent to the secretary of the board of health and are investigated by the agent, as it obviates the necessity of the secretary writing letters to the agent in regard to complaints, compels the agent to put the results of his investigations in writing, encourages promptness on the part of both secretary and agent, and furnishes a convenient method of filing.

We have a numbered stub on which are spaces for recording the date on which each complaint is made, the name of the person making it, the nature of the complaint, location of property, name of owner and occupant, the date when the receipt of the complaint was aknowledged, when it was sent to the agent, when the coupon was received from him and when acted upon by the board, and, finally, a space for recording the result. On the numbered coupon we inform the agent of the nature of the complaint, the location of the property, the names of owner and occupant, and request him to investigate the matter at once, and to write his report on the back of the coupon, and return it to the secretary.

We have had trouble in Milton in getting people to make written complaints. They would call attention to nuisances verbally on the street or telephone about them. This gave rise to more or less misunderstanding; and, consequently, we have prepared blanks which are issued in blocks of twenty. One of these blocks is carried by each member, and the agent of the board of health, and the chief of police, so that the essential facts in regard to any nuisance, and the name of the party responsible for making the complaint, can be readily secured.

# JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH

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## October Meeting, 1900

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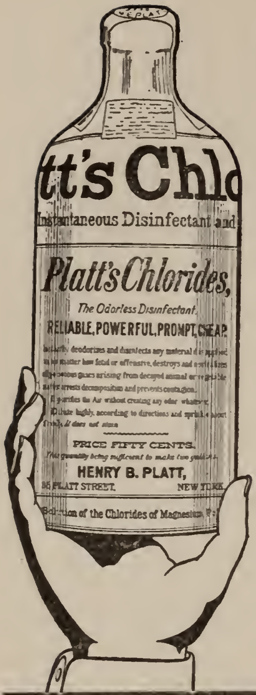
SUBJECTS: The Swab Method in Rapid Diagnosis of Diphtheria — Glanders — The Necessity for More Thorough Inspection of Sanitary Conditions in Massachusetts

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## THE JOURNAL OF THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH was organized in Boston in March, 1890, with the following objects: the advancement of sanitary science in the Commonwealth of Massachusetts; the promotion of better organization and co-operation in the local Boards of Health; the uniform enforcement of sanitary laws and regulations; and the establishment of pleasant social relations among the members of the Association.

All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

THE OFFICIAL JOURNAL OF THE ASSOCIATION is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them.

All communications to the Association should be addressed to the Secretary, **EDWIN FARNHAM, M.D., City Hall, Cambridge, Mass.**

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## OCTOBER MEETING

OF THE

## Massachusetts Association of Boards of Health.

The October quarterly meeting of the Association was held at "The Johnsonia," Fitchburg, on the afternoon of Oct. 18, 1900. A banquet, complimentary to the Association, was furnished by the municipal government of Fitchburg. Just before the close of the banquet Dr. Walcott, who presided, called the assembly to order, and spoke as follows: —

The President.— Gentlemen of the Association, it is rather necessary that we should begin; and, fortunately, the beginning is not going to be one of that sort which will disagree with your food or the taking of it. His Honor, the Acting Mayor of Fitchburg, will say a few words to the Association. I have the pleasure of introducing him to you,— Mr. George H. Priest. [Applause.]

Mr. Priest.— Mr. President and members of the Association, I stand here, in the absence of Mayor Anderson, to give you a brief word of greeting at this time. I consider it an honor that Fitchburg should entertain within her gates such a representative body of men as I see before me. Fitchburg does welcome you at this time; and

we trust that, before you get through your meeting and finish your business, you may settle for us the vexed question of our new sewage plant. Our local Board of Health wants to know something about that, I believe; and I see also a couple of members of the Commission here who would be greatly pleased to have the question decided for them. [Applause.]

Owing to the lack of time, the reading of the records of the last meeting was dispensed with.

THE PRESIDENT.— Is the special committee on "Diphtheria Bacilli in Healthy Throats," of which Dr. Chapin is chairman, prepared to report at this time?

DR. CHAPIN, Providence.— Mr. President, if I remember right, our committee was to report at this meeting; but, unfortunately, we found that we needed to spend a great deal of time in collecting data in regard to the presence of diphtheria bacilli in the throats of the well. Dr. Hill, the secretary of the committee, has been very busily engaged in this work. He sent out letters of inquiry, 192 in all, of which 134 were sent to cities, 39 to State Boards of Health, and 19 to hospitals. Replies have been received from 74. These schedules of inquiries which were sent out were quite elaborate, and the labor of tabulating the replies is very considerable. As all of the replies that we expect have not yet been received, it was found impossible to have a meeting and discuss the question as fully as it should be discussed. Hence we are not able to report to-day, except to report progress. If it is your pleasure to continue the committee to the next meeting, we hope to be able to present a final report at that time.

THE PRESIDENT.— Shall your committee have further time? If there is no objection, it will be so understood. There is a piece of delayed business which I shall have to apologize for not presenting first, and that is the report of your Ex ecutive Committee. The Executive Committee report for recommendation for election to membership in the Association the following names:—

| | |
|---|---|
| J. N. Marston, M.D. | Lowell. |
| B. R. Richards | Boston. |
| J. P. Schneider, M.D. | Palmer. |
| J. Sanford Orr, M.D. | Weston. |
| Clarence P. Holden | Melrose. |
| William H. Dole | Melrose. |
| Paul H. Provandie, M.D. | Melrose. |
| C. E. A. Winslow | Boston. |
| Arthur B. Wetherell, M.D. | Holyoke. |
| Frank A. Woods, M.D. | Holyoke. |
| T. J. Linehan | Holyoke. |

The above-named gentlemen were elected members of the Association.

The President.— The first paper on the programme is on "The Results of One Year's Use of the Swab Method in Rapid Diagnosis of Diphtheria," by Dr. Atherton P. Mason, of the Fitchburg Board of Health.

## THE RESULTS OF ONE YEAR'S USE OF THE SWAB METHOD IN THE RAPID DIAGNOSIS OF DIPHTHERIA.

BY ATHERTON P. MASON, M.D., BACTERIOLOGIST OF THE FITCHBURG BOARD OF HEALTH.

At the meeting of this Association held a year ago, at Leominster, Dr. H. W. Hill read a paper on "Rapid Diagnosis of Diphtheria by Direct Swab Examinations," which was of great interest to me. I had already tried the method in a small way; but, not being able to tell, with sufficient degree of certainty whether the swab cultures contained diphtheria bacilli or not, I had not found it entirely satisfactory. Dr. Hill's paper, however, cleared up several doubtful points and gave me renewed courage; and on Nov. 1, 1899, I put the method into use here in Fitchburg, and have continued it since that date with fairly satisfactory results.

Preliminary to this paper, which will be but a brief summary of cases in which the swab was used for diagnosis, and some remarks

on certain points connected therewith, I desire to say a few words as to my method of procedure. In the first place my copper boxes differ slightly from any others I have seen, and were made from my own design, by Mr. Peter Gray, of Boston, and have proved excellently adapted to this method of diagnosis. Each box is $6\frac{3}{4}$ inches long, 2 inches wide, and 1 inch deep, and is divided into two compartments by a partition, $\frac{1}{3}$ of an inch thick, having a groove on the top. This partition does not run the whole length of the box, but stops about an inch from the top end, thus giving abundant room for the flanges of the test-tubes. Each box fits into an ordinary copper case. When the box goes out for a diagnosis case, it contains the following: a tube of serum in one compartment, a swab tube in the other, and a platinum wire in its holder in the groove on the partition. With the paper for data of the case is put a slip, asking the physician to first take a wire culture on the serum and then rub the swab over the affected part and replace in the tube for rapid diagnosis. The box is sent to me as soon as may be after the culture is taken, the serum tube put into the incubator, and the swab immediately examined and reported on. I am a firm believer in wire cultures; and my box allows of having the serum culture taken once for all in that way, and leaves the swab culture to be used *solely* for immediate examination. This is the second point of difference between the ordinary mode of procedure and mine, and it seems to me to enhance the chances of correct diagnosis from the swab. If the swab seems unquestionably positive at the first examination, that suffices; but, if the first preparation from the swab is negative or doubtful, I make another, and, if necessary, a third or even fourth preparation before deciding. The wisdom of doing so was shown me in two cases during the past year, to be spoken of later, in which, being pressed for time, I relied on *one* preparation from the swab.

From Nov. 1, 1899, to Oct. 15, 1900, 126 primary serum cultures were examined at my laboratory. Of these, 14 were primary in the sense that they were the first cultures sent me from the cases, but were ostensibly for release, no culture having been taken at the beginning of the cases. These were, of course, unaccompanied by a swab. There were also 13 cases for diagnosis in which no swab was used. Eliminating these 27 cases, we have 99 cultures from 97

persons (in 2 cases cultures from both throat and nose being taken from each patient), in which both swab and serum cultures were sent in. These 99 cultures, therefore, form the basis of this paper; and the following summary gives the result of my examination of them as reported to the Board of Health: —

| | Cultures. | Cases. |
|---|---|---|
| Swab and serum both positive | 37 | 35 |
| Swab and serum both negative | 34 | 34 |
| Swab negative, serum positive | 10 | 10 |
| Swab positive, serum negative | 3 | 3 |
| Swab doubtful, serum positive | 7 | 7 |
| Swab doubtful, serum negative | 6 | 6 |
| Swab doubtful, serum no growth | 2 | 2 |
| Total | 99 | 97 |

This shows that in 71 cultures out of 99, the swab and serum agreed, being 71.7 per cent.; in 13 cultures there was total disagreement between swab and serum, 13.1 per cent.; while the remaining 15 swabs were doubtful, the serum cultures proving positive and negative about equally. These figures are interesting; but, as negative swabs are unreliable, the only point of really practical value to us is the proportion of positive cases diagnosticated from the swab. 54 of these serum cultures proved positive; and of these 37 were positive by swab, being 68.7 per cent. In cases of positive or doubtful swab, where serum is negative or shows no growth, it is my habit to ask for a second culture. The request has been complied with in 6 of the 18 cases of the kind that have occurred during the past year. In 2 of these second cultures no swab was used; and the only important change shown by the remaining 4 was in a case of swab positive, serum negative, the second culture showing both positive. This adds one more to the number of positive cases diagnosticated from the swab, making 38 out of 54,— just 70 per cent.

In several cases of negative or doubtful swab, where the serum culture showed comparatively few bacilli,— and those largely obscured by growth of other organisms, so that it was impossible to be absolutely sure, by ordinary staining, that they were diphtheria bacilli,— I have found Hunt's stain very valuable. It takes but a few minutes to do it, and generally settles the question decisively; while a plate culture or second culture from the patient would require some hours.

The results of the use of the swab the past year are more accurate than was anticipated, and can, it seems to me, be reasonably attributed solely to the fact that the swab may be used for making two or three preparations, if necessary. I alluded earlier in this paper to 2 cases where I reported a negative swab after examining only one preparation from it. A few hours later I examined the preparation again with the same result. Then, in accordance with my custom, I made a second preparation in each case (the 2 cases were not at the same time, but several months apart); and examination in each instance showed positive evidence of diphtheria bacilli, so I had to reverse my decision. The serum cultures proved positive later in each of these cases. Had I delayed reporting for a few hours, these 2 would have increased the number of positive cases diagnosticated from the swab to 40 out of 54,— 74 per cent.

I very much regret my inability to furnish comparisons between swab diagnosis and regular five and fifteen hour incubation of serum cultures. Being able to devote only a portion of my time to bacteriological work, I find it impossible to carry out this method. Serum cultures that come in during the day or evening are always examined on the morning of the following day, the period of incubation ranging from twelve to twenty hours.

There is one interesting fact which I desire to mention, as it is in the line of the report of the committee just read by Dr. Chapin. 15 of the above enumerated cases of swab and serum cultures were taken from persons not ill, but whose throats were examined because of either having been exposed to diphtheria or being suspected of having bacilli and causing contagion. Of the 15 swabs used, 12 were negative, 2 positive, and 1 doubtful. 5 of the serum cultures proved positive. One of these positive cases was that of a travelling salesman, and he was immediately quarantined on the swab diagnosis. Here is where the value of this method, if due care and experience are associated with its use, is most plainly shown; for it renders possible a very early isolation of the case, and very much decreases the chances of allowing the spread of the disease to others. One case in particular I recall which occurred last January. A man, who kept a grocery store on the lower floor of a tenement house occupied by something like one hundred persons, was taken ill, and sent

for a physician. There were no special throat symptoms, and the physician was not particularly suspicious of its being diphtheria; but, under the circumstances, he deemed it prudent to take a culture and send it to me for examination. I found the swab unquestionably positive, and the case was quarantined immediately. Sometimes, however, I have found out, to my own discomfiture, it is not safe to quarantine on the swab. In 2 cases I made an egregious blunder with the swab, calling it positive when the serum cultures persistently in first and second instances failed to show anything but a miserable streptococcus growth.

Undoubtedly, work of this kind — which is at the present time, to me at least, in a large degree experimental — is open to criticism and improvement. Lack of experience has, as might be expected, caused me to make some mistakes; and I have made this paper as brief as possible, so that there may be more time for discussion of the subject by those who have a wider experience.

It would seem, however, that, if physicians and bacteriologists worked together in perfect accord, and strove to carry out this method carefully, extremely few cases of diphtheria would escape detection. The only loophole in the system as carried on at present is where swab and serum are both negative and no further cultures made.

The success or failure of bacteriological work depends a great deal on the care, intelligence, and interest shown by the physicians who obtain material for examination; and, in closing this paper, I desire to thank the physicians who, by putting themselves to the trouble of taking swab cultures for diagnosis, have enabled me to prosecute this investigation, and hope that the results attained may be sufficiently valuable to insure a continuance of this method.

The President.— The next paper on the programme is on "Glanders," by Dr. Alexander Burr, of Boston.

## GLANDERS.

BY DR. ALEXANDER BURR.

*Mr. President and Gentlemen*,— While it is true that the municipal boards of health of this State, with the exception of the Boston Board of Health, have at present nothing to do officially with glanders in horses, the method of the diagnosis of glanders is a subject of some scientific and practical interest to this Association, especially in view of the occasional occurrence of this disease in man.

This disease is known in animals as glanders and farcy. They are one and the same disease, due to the same organism. The term "glanders" is applied to this disease when the external lesions are present in the nasal mucous membrane and adjacent lymphatic glands, as the submaxillary gland. The term "farcy" is applied to this disease when the external lesions appear as ulcers in the skin, accompanied by a nodular condition of the superficial lymphatics. The existence of the two terms, "glanders" and "farcy," is misleading, since they are often considered to mean two distinct diseases. It is much better to speak of the disease as glanders, without regard to location. That they are one and the same disease there can be no doubt, as the bacillus mallei is the cause of both. Experimentation has demonstrated that inoculation from glanders may produce farcy, or glanders; and the same is true of farcy.

Glanders occurs clinically in horses, mules, and asses. It occurs in man probably by transmission from animals affected with glanders. Cattle appear to be immune.

The mode of infection is probably by direct contact, as from a diseased animal, or places and articles which have become infected by glandered animals, as harnesses, stable fittings, cleaning instruments, watering-troughs, hitching-posts, etc. It gains entrance ordinarily through an abrasion on the nasal mucous membrane or the skin, possibly through the digestive system.

Glanders occurs in animals as an acute or chronic disease. The usual form seen is the chronic.

In acute glanders there is marked constitutional and functional disturbance, with intense fever, often running as high as 105 or 106 degrees, which lasts for a number of days. There is rapid emaciation, frequent chills, and perhaps lameness without any apparent cause, followed by œdema of the extremities. While in this stage, which lasts two to four days, the diagnosis is very difficult to make. Soon the fever begins to diminish, followed by local lesions. Glanders nodules appear on the nasal septum, which rapidly soften, causing more or less discharge, and leaving a deep, ragged edge ulcer. The disease in a short time becomes general in the lungs and lymphatic glands, with intense inflammation of the synovial membranes of the joints. Fever now begins to increase again, cheesy deposits in the lungs become extensive, and death follows, in from one to two weeks.

Chronic glanders probably always begins with a slight acute attack of glanders, which is not recognized as such, probably being diagnosed as a slight coryza, or an attack of lymphangitis. In fact, the history often obtained in chronic glanders is that the animal has had a number of attacks of lymphangitis, which readily yielded to treatment, the horse returning to work and continuing for some time in good condition.

Chronic glanders may continue for months and even years before definite symptoms, such as typical ulcerations of the mucous membrane of the nose or the subcutaneous tissues, appear to warrant one in making a positive diagnosis. I have known a case, from the history given at the time of examination, to have been as long as two years in duration. On Tuesday of this week I held an autopsy on a horse which I have had under observation for over a year. It was one of a four-horse team, its mate — that is, the horse which worked side of it — and one other horse having been killed as cases of glanders. Upon examination of the entire stable this horse — a fine, large 1,600 pound animal, in apparently perfect health — showed a slightly enlarged submaxillary gland, together with a slight mucous discharge from one nostril. Although the symptoms shown were not very suspicious, the history of previous cases was enough to warrant further examination of the horse. It was subjected to the mallein test, swab preparations were made from the nasal discharge, and

guinea-pigs were inoculated from the swabs. All mallein tests were positive, and all guinea-pig tests negative. As the guinea-pig tests proved negative, no definite action could be taken, with the result that the horse was put to work under well-defined restrictions. Tests were made from time to time with the same results as before. Clinical symptoms have been about the same, and at no time were sufficient to warrant the killing of the horse, in view of the fact that the guinea-pig tests opposed such action. About three weeks ago a swab preparation made from the nasal discharge, which had somewhat increased, was inoculated into a guinea pig, giving a positive result. This has since been repeated three times, two of which proved positive, making three positive out of four tests within three weeks. Upon autopsy, well-marked miliary lesions of glanders were found throughout both lungs.

I mention this case to show the chronic nature of the disease. This form of the disease in horses simulates chronic tuberculosis in man more closely than any other disease of man. The period of inoculation, as in tuberculosis, is indefinite. It is an exceedingly difficult disease to eradicate or even to hold in subjection. This is due to the fact that horses in apparent health may be affected with glanders for months and years, during all of which time it undoubtedly is a source of infection to others. In attempting to eradicate glanders in a large stable, very often the last case discovered may prove to be the cause of all previous cases. An instance of this occurred in one of our large railroad stables in Boston, where glanders kept cropping up from time to time in all parts of the stable. For a long time the cause could not be found. It was apparent that it must be due to some animal which had access to all parts of the stable. An old horse which had outgrown its usefulness, although in seemingly good health, was used for pulling the feed-box about the stable, thereby coming in contact with all the other horses and stalls. It seemed advisable to kill this horse; and, upon autopsy, well-marked miliary lesions of glanders were found throughout both lungs. Undoubtedly, this horse was the cause of the previous cases, although presenting no symptoms in himself.

The usual symptom of chronic glanders is a nodular condition of the mucous membrane of the nose, generally on the nasal septum.

These nodules vary in size from a pin-head to that of a pea. Later these nodules ulcerate, leaving a raw surface, which has a raised irregular edge. Several ulcers may run together, forming a large one. There is very little tendency in these ulcers to heal. They continue to discharge a thick, gluey, muco-purulent discharge, which adheres to the external edges of the nose. There is no odor, excepting when necrosis of the bone occurs. Occasionally they heal, leaving a rough, drawn scar. One or both of the submaxillary glands are usually enlarged, hard and nodular, more or less adherent to the inferior maxilla, and with very little tendency to suppurate. Lymphatic glands all over the body may become enlarged. Nodules followed by ulceration may appear upon any part of the skin, usually the legs. The discharge from ulcers upon the leg is a semi-translucent, thick, amber-colored pus, sometimes occurring as clear serum.

Glanders has long been recognized as an infectious disease; but the cause of its infectiousness was not discovered until 1882, when Loeffler, Schultz, and Israel found the bacillus in glanders tissue. The bacillus mallei is about one and one-half times the size of the tubercle bacillus, varying in size on different medium, having rounded ends, and frequently showing unstained areas. They have no characteristics either of form or staining by which they may be distinguished when prepared direct from a suspected discharge. On potato in about thirty-six hours in incubator, it appears as rather thick, viscid, drop-like colonies, which soon become brownish or amber-colored. Its growth upon potato, together with the peculiar lesions produced when inoculated into male guinea pigs, are the only means of identifying this bacillus. It is found in all lesions of glanders, and sometimes in the blood in small numbers of acute cases.

The method of guinea-pig diagnosis is of considerable interest, because of the fact that cases are seen upon which it is impossible to make a clinical diagnosis. Having a case with one or both submaxillary glands enlarged, nasal discharge, and typical ulceration of the nasal septum, the diagnosis is perfectly clear; and other methods of diagnosis are unnecessary. Cases, however, are frequently met with where the only lesion to be seen is an enlarged submaxillary gland, others where only a slight nasal discharge is present, and still others where the only lesion is an ulcer of the skin. In such cases the clin-

ical diagnosis must be doubtful. Formerly such cases were allowed to remain among other animals until the symptoms had advanced enough to warrant killing. Now we have at hand the inoculation test upon guinea pigs and the mallein test, as aids in early diagnosis. Having a suspected discharge, an emulsion is made in water, and about two cc. are injected into the abdominal cavity of a male guinea pig. If the case is glanders, in from one to three days there appears a swelling of one or both testicles, which later become firmly adhered in the scrotum. When this stage is reached, the pig is generally chloroformed and autopsied. Upon autopsy there is found a nodular deposit of cheesy-like matter upon the visceral layer of the *tunca vaginalis*, which in the guinea pig is continuous with the peritoneum. The nodules extend into the testicles: the testicles are enlarged and become firmly adherent by these nodular deposits to the scrotum. The glanders bacillus may be detected in cover glass preparation made from the direct nodules. Cultures are made upon potato from the cheesy matter of these nodules, and after incubation the glanders colonies are readily distinguished by their characteristic brownish color. It is always well to make cultures upon potato, and not rely wholly upon the guinea-pig lesions, as occasionally enlargement of the testicles, though usually of only one, may be due to some other organism. This has been observed in the work of the Boston Board of Health laboratory in two or three instances; but each of these cases presented on autopsy an abscess of the testicle rather than a nodular condition, and the character of the abscess was readily distinguished by potato culture later.

Special outfits for use in glanders may be obtained at all of the diphtheria stations in Boston. This outfit consists of a cylindrical copper box, containing an 8-inch test-tube, a card for particulars to be filled out by the veterinarian, and a circular of directions. The test-tube contains a large cotton swab, similar to that used in diphtheria work. No medium such as is sent out in diphtheria outfits is used, as the object of the glanders outfit is to convey the suspected material to the laboratory to be used for guinea-pig tests.

This method of diagnosis is now well organized, and has been available to all veterinarians of the city for the past two years.

Upon receiving the outfit at the laboratory, an emulsion is made by

soaking the swab in water. You can readily see that such an emulsion contains usually a mixture of organisms, especially if the swab is from the nose. This mixture may or may not contain the glanders bacillus. If the glanders bacillus is not present, and no other pathogenic organisms in sufficient numbers or of sufficient virulence to produce death are present, the guinea pig, after a few hours or days of depression, recovers completely. If the glanders bacillus be present, and the other organisms also present fail to kill, the typical scrotal lesions or the peritoneal lesions will develop, the former within twenty-four hours to seventy-two hours usually, the latter later on. If, however, acute septic organisms be present in the injected liquid, the guinea pig is likely to die within twenty-four hours, whether the glanders bacillus be present or not, before the glanders lesions have time to develop. In such cases the test is evidently of no value, and should be repeated. Such a result certainly fails to demonstrate the presence of the glanders bacillus, so that a positive report is impossible. It also fails to demonstrate its absence, since experimentally it has been shown that a guinea pig injected with a pure culture of glanders mixed with a pure culture of the pneumococcus (the latter isolated from a case of acute peritonitis in a guinea pig injected for diagnosis) may die within twenty-four hours without glanders lesions. A negative report based on such a case is evidently out of place; and therefore an "unsatisfactory result" is reported, accompanied with a request for another preparation.

During the year 1899, 46 cases have been examined by guinea-pig tests, 23 of which were positive, 14 negative, and 9 unsatisfactory. 5 of these unsatisfactory cases later proved to be glanders clinically.

I have here a table showing comparison between clinical diagnosis and guinea-pig tests: —

TABLE NO. V.—GLANDERS.

SHOWING COMPARISON BETWEEN CLINICAL DIAGNOSIS AND GUINEA-PIG TEST.

| *Clinical Diagnosis (in Horse).* | *Guinea-pig Test.* *Pos.* | *Neg.* | *Unsat.* | *Total.* |
|---|---|---|---|---|
| Positive | 11 | 2 | 5 | 18 |
| Negative | 1 | 4 | 2 | 7 |
| Doubtful | 12 | 8 | 1 | 21 |
| Total | 24 | 14 | 8 | 46 |

Hence 57 per cent. of those cases which the attending veterinarian could not decide from clinical evidence at the time were shown to be glanders by the guinea-pig test. These cases, in the absence of the guinea-pig test, would have remained amongst other horses until typical clinical symptoms developed, acting as sources of infection in the interim.

The mallein test is similar to the tuberculin test for tuberculosis in cows, and consists in the injection of a small quantity of mallein, subcutaneously, into a suspected animal, with the result that, if glanders is present, there occurs a high rise of temperature, to 104 degrees F, or more, with a well-marked swelling at the point of injection. If glanders is not present, very little variation of temperature occurs.

Mallein is prepared in about the same manner as tuberculin. Bacillus mallei, grown on agar at body temperature for forty-eight hours in an incubator, is inoculated into a flask of ordinary nutrient broth, to which has been added 5 per cent. glycerine, and the reaction made acid (1.5 per cent). This is allowed to grow in the incubator for about a month. It is then concentrated by boiling to about one-half its bulk, and afterward filtered. One-half of 1 per cent. of carbolic acid is added, and mallein is ready for use in about 1 cc. doses.

The method of testing with mallein is as follows: —

Having by previous examinations ascertained what the normal temperature of the suspected animal is, a subcutaneous injection is made in the thick portion of the neck. The reaction to mallein consists in a high rise of temperature with more or less swelling at the point of injection. The rise in temperature occurs usually in from six to eight hours, requiring the temperature to be taken within about six hours, and repeated thereafter about every three hours. A swelling at the point of injection is always present; but, if glanders is present, it is large, well marked, with abrupt edges. If glanders is not present, the swelling is small and flat. The usual result where a reaction has taken place is as follows: temperature at time of injection, 10 P.M., 101; 5 A.M., 104; 7 A.M., 105; 9 A.M., 105; 11 A.M., 105; 5 P.M., 105; in forty-eight hours, probably back to 102. During the test, no water is to be allowed, but food to be given as usual.

Mallein has been used by this department to a considerable extent, particularly in such cases as were doubtful from a clinical standpoint, and where material for guinea-pig test was not available, as, for instance, where the only lesion to be discovered clinically was an enlarged submaxillary gland with no nasal discharge. Where it has been possible, all horses which have been associated with another animal having glanders, as, for instance, a mate working in the same team with a glandered horse, have been subjected to the mallein test. In such cases, where no symptoms are present and a reaction is made to mallein, such horses have been kept under strict observation by the Board of Health.

Such varied opinions have been expressed by different authorities in regard to the value of mallein as a diagnostic agent, it has been thought well to tabulate the last one hundred cases as showing the actual results obtained in practice.

The following table shows, in a condensed form, the relation of the mallein test to the clinical diagnosis at the time of making the mallein test, and to the final diagnosis based on autopsy, guinea-pig test, or prolonged observations: —

| 1. | | 2. | 3. | | 4. | 5. | | 6. | 7. | |
|---|---|---|---|---|---|---|---|---|---|---|
| Clinical Diagnosis. | Totals. | Mallein result positive. | Final clinical diagnosis on positive mallein tests. | | Mallein result negative. | Final clinical diagnosis on negative mallein tests. | | Mallein result doubtful. | Final clinical diagnosis on doubtful mallein tests. | |
| | | | Pos. | Neg. | | Pos. | Neg. | | Pos. | Neg. |
| Positive . | 38 | 36 | 36 | — | — | — | — | 2 | 2 | — |
| Negative . | 31 | 4 | 2 | 2 | 25 | — | 25 | 2 | — | 2 |
| Doubtful . | 31 | 25 | 20 | 5 | 2 | — | 2 | 4 | 2 | 2 |
| | 100 | 65 | 58 | 7 | 27 | — | 27 | 8 | 4 | 4 |

Column 1 shows the one hundred cases divided into positive, negative, and doubtful on clinical diagnosis.

Column 2 shows the number positive mallein tests obtained from each of these divisions.

Column 3 shows the final diagnosis of the animal giving positive mallein test.

Columns 4 and 6 give similarly the negative and doubtful mallein results.

Columns 5 and 7 show the final diagnosis on these respectively.

The conclusions drawn from this table are as follows: —

1. All animals, whether suspected of glanders or not, failing to react to the mallein test, may be considered free from glanders. (See columns 4 and 5.)

2. Animals, whether suspected of glanders or not, reacting to the mallein test, may or may not be affected with glanders. (See columns 2 and 3.) It will be seen, however, that 89 per cent. of the animals reacting to the mallein test are affected with glanders.

3. Animals giving a doubtful reaction to the mallein test may or may not be affected with glanders. (See columns 6 and 7.)

Attention is called to the fact that the conclusions are drawn as a result of the above one hundred cases.

The results of the mallein tests have been classed as doubtful in the above table when the temperature before testing was 102 or less, and the temperature during the test failed to reach 104. If the temperature before testing was above 102, the mallein test is also considered doubtful unless a rise of two degrees or more occurs during the test.

I have found the negative mallein reaction of much greater value and more to be relied upon than a positive reaction, contrary to experience in other diagnostic tests where the positive is of more value than the negative. I have never had a negative mallein reaction upon a horse which afterward proved to be glandered, and the total mallein cases now number about two hundred. Positive results are less reliable, as there is little doubt but that horses free from glanders will often react to mallein. While glanders may quite likely be present, you are not warranted in killing upon the positive mallein alone. Doubtful reactions are of no value.

If called up to examine a horse which presents no positive symptoms of glanders and a negative mallein reaction is obtained, one feels quite safe in making a diagnosis of not glanders.

Mallein may now be obtained free at the laboratory of the Boston Board of Health by any veterinarian for use upon animals in Boston.

Glanders in man runs a course very much like acute glanders in animals. The mode of entrance is probably through an abrasion in the skin. An early diagnosis cannot be made; and, in fact, diagnosis can only be made by guinea-pig test from some suspected area, as an ulcer upon some part of the body or incision into œdematous tissue about a joint.

I remember a case which occurred in one of the hospitals in Boston which was suspected of having about all the febrile diseases before a positive diagnosis of glanders could be made. Typhoid, pleurisy, and rheumatism were suspected, until some symptom appeared to offset each of these diagnoses in turn.

In man, glanders is usually a fatal disease, death occurring within three weeks.

Although glanders is a fatal disease in man, the bacillus must usually be of low virulence to man, since very few cases occur in spite of the fact that there is no infectious disease which is handled by a certain class of men with less precaution. It is not uncommon at all to be called to a case of well-marked glanders in an animal, and find a stable groom and the owner dressing the horse, with cuts upon their hands and no disinfection afterward. Veterinarians are often obliged to handle these cases in a very careless manner, as means of disinfection are seldom found in stables. Men who cut up glandered horses at rendering places are seldom without a cut or two upon the hands, still glanders rarely results.

The President.— The next paper on the programme is entitled "The Necessity for more Thorough Inspection of Sanitary Conditions in Massachusetts," by Dr. W. S. Everett, of Hyde Park.

## THE NECESSITY FOR MORE THOROUGH INSPECTION OF SANITARY CONDITIONS IN MASSACHUSETTS.

BY DR. W. S. EVERETT.

It may seem like presumption for one who has sat for years,— a willing and to some extent, at least, an appreciative listener to the many valuable and suggestive papers that have been presented here, who cheerfully acknowledges his indebtedness to those papers and the equally valuable discussions by which they have been followed for much of profitable instruction,— it may, let me repeat, seem like presumption for such a one to come in before this Association, and attempt to persuade its members into the belief that the work which they are especially appointed to accomplish is not thoroughly performed.

But whoever reads newspapers, whoever keeps abreast with the medical history of the times, whoever has made himself familiar with existing conditions within the limits of the State, whether as members of boards of health or not, is perfectly well aware that there are yet many places within our borders where the simplest laws of health seem to be either ignorantly disregarded or set at defiance by some motive that is more to be deplored than ignorance and that cannot be so easily excused.

And it casts no reflections upon either the efficiency or the public spirit of existing boards of health to admit that this is true. There are causes that are amply sufficient to make it impossible for them to accomplish more in this direction than is now done. But, that there is urgent need that more should be done, probably no one who is well informed entertains a doubt.

But in what way a better service can be obtained or rendered is a problem that is disturbing the peace of many thoughtful minds.

Some time in August last an article appeared in some of the daily papers, stating that the honored President of this Association — in view of the sudden outbreaks of disease that sometimes appear in different localities within the limits of the State — had recommended

that a committee — to consist of some thirty members or thereabouts, perhaps — should be appointed, whose duty it should be to make a more rigid examination of the primary causes of these sudden outbreaks and epidemics, and the best method of dealing with them, and also of dealing with unsanitary conditions generally, than is possible in conditions that at present exist.

It was stated distinctly that the purpose of this committee should not be to supersede or interfere in any manner with the work or the duties of the local boards of health, but rather to co-operate, while pursuing an entirely separate and independent method of investigation, with existing boards, and to search out for themselves the places where disease-laden germs might be expected to propagate and multiply, and simply to report the result of their investigations, with such suggestions as might be deemed fitting, to the local boards whenever cause for such reports should be found, and also to make their report to the State Board, as occasion should require.

This recommendation is far-reaching, and it would seem that it must go a long way toward reaching and surmounting many of the difficulties and obstacles that beset the way.

A few days after this article appeared, a request was received by the writer of this paper from the *Advertiser*, asking that he would write out for publication in its columns a statement of his views concerning the expediency or necessity for the appointment of such a committee as had been recommended.

Previous to that time the special need for such a committee had not been considered. But, when attention was called to it, first impressions were favorable to such an appointment; and subsequent and maturer reflection has only strengthened the conviction, and fortified the opinion, not only that there is need for such a committee and that there is ample and useful and profitable work for it to do, but that such a measure is deserving of all commendation, encouragement, and support. But individual opinions amount to but little. It is the general consensus of public sentiment that must be relied upon to sustain it; and, if it is deemed to be worth a trial, that must be invoked to yield to it a cordial support.

And so, before this measure gets any farther, while yet in its embryonic state, the probabilities and the possibilities resulting from its operation should be thoroughly and fully considered and made plain.

Whatever advantages its advocates are prepared to claim from its operation should be brought fully and fairly into public view. And, if there are objections to be urged against it, they should be made clearly to appear.

It is a measure of no small moment to the people of this commonwealth. For one of two things will result from its adoption. Either a great and lasting improvement in its hygienic condition will be secured to the State, thereby insuring a permanent benefit with which no pecuniary consideration can be compared, or otherwise the State will be burdened with an additional and considerable expenditure to support a useless, unnecessary, and impolitic, if not dangerous scheme.

It seems appropriate that a sanitary measure of so much importance should be brought squarely before this Association. Dedicated, as it is, to "the advancement of sanitary science in the Commonwealth of Massachusetts," it should be fully considered here; and, if it is decided to be a step in advance in the great problem of public health, it should not only receive its initiatory impulse, but its progress onward from a theoretical project till it shall be expanded into a fully developed law of the land should be directed by a no less influential and intelligent and authoritative body than the Massachusetts Association of Boards of Health.

The place where such a commission as is contemplated in this recommendation would have its greatest and best opportunity to be of benefit to the community would be in determining and locating the ultimate sources of danger at the places of their origin, before those dangers had developed into actual fact, and before it is usual for them to attract the attention of any one else. And, manifestly, something is wanting here to make our present system of hygienic sanitation complete. There is work here that should be done by somebody that has never yet been attempted in the right way, and in conditions at present existing is likely to remain undone to the end.

Some different management must originate somewhere, some more effectual method must be devised of dealing with this subject, if the elements of danger are ever to be eliminated from our midst.

Epidemics of typhoid fever appear in various localities; and there

is every reason for entertaining the gravest suspicion that the causes from which they have taken their origin were preventable, and might have been removed if proper action had been taken at the right time.

Visitations of diphtheria come upon us like an avalanche, and surprise us by being directly traceable to the products of a single dairy herd. And, worst of all, cupidity and greed are found to be more potent than all other considerations; and milk is sent to market to be used in families, all unsuspecting of danger, that is drawn from the udders of cattle that have not been relieved of their burden for forty-eight hours — and more.

So much we know. And these things are happening, not alone among the indifferent and unconcerned residents of neglected places, but are only of too common occurrence, even in communities where the search-lights of science are blazing forth their brightest rays. And, while these things are possible, the work of the sanitarian among us cannot be regarded as an altogether unqualified success.

And if, as there is too much reason to fear from reports that come to us, there are those contributing to the public milk supply who — either through ignorance, carelessness, thoughtlessness, laziness, indifference, or for some other reason — are keeping their cattle in dark and damp, foul-smelling, ill-ventilated, overcrowded quarters, who have regard neither to cleanliness nor wholesomeness in the methods of their milk production, and who are keeping their stock with reference to the drain upon the bank account rather than the quality of the product that they are sending to the markets, they must be made in some way to feel the moral accountability that is resting upon them when an epidemic breaks out among their patrons, or, if this is found to be impossible, must be made to realize their legal responsibility to the laws of the land. Whatever else may happen, those who are so unmindful of the plainest requirements of sanitary principles must not be permitted to mingle their products with that which is produced in the most careful and painstaking manner, and by thus contaminating and polluting the whole output bring disgrace and liability to prosecution upon all alike.

And, if there are people whose water supply is polluted by surface drainage or by percolation from some near-by privy-vault, or whose

sink drain forms a direct conductor to their dwellings from some adjacent miasmatic or poisonous slough, they must be made to comprehend the fact that they are living in the midst of dangers compared with which proximity to some dynamite depositary is less to be feared.

If there are stagnant pools and waste places in outlying regions that are polluting the air with miasm and contagion, they must be sought out and reclaimed to uses that, at least, shall not be a menace to health.

Science can do much — has done much already, and is constantly doing more and more — to teach us to recognize these dangers and to indicate the direction from whence protection must come.

It has done much to make it hazardous for the enterprising but unscrupulous dairyman either to attempt to increase the quantity or tamper with the quality of his dairy product, and so augment his revenue by artificial means.

It has taught us that water may be unfit for domestic uses, and yet give no sign to taste or sight or smell.

It has taught us that the bacillus of disease may linger in waste places, and yet give no warning to any material sense. It has taught us that the most villanous odors are not always the most dangerous; and it has also taught us that the festive microbe may be not only quietly marshalling his aggressive forces to assail his unsuspecting victim in undisturbed tranquillity in swamps and marshes or in the earth's most pleasant places, but may also be joyously luxuriating in riotous excesses in those beautiful spots where the landscape gardener has exhausted the resources of his art, where the air is laden with delicious fragrance,— where the rose and the violet, the lily and the sweet-brier, abound.

It has done all this. But there are elements of danger which science cannot reach. It cannot distinguish and separate the integral particles of pure milk from those that are not fit for use, when once they have been mingled together; and the service of just such a commission as is here contemplated is imperatively demanded to make us sure that the pure and the impure products shall never come in contact with each other.

It cannot determine whether cattle are properly housed and fed

and cared for, whether barns are cleanly, or whether cans and jars are so thoroughly cared for that they are safe and proper receptables for a milk supply. The service of just such a body as is here contemplated is constantly needed, whose careful inspection and unremitting watchfulness shall be a sure protection against the violation of sanitary requirements, in these particulars, in all places, and at all times, even though, to secure so great an object, persistent house to house inspection and observation should be required.

It cannot extend the shield of its protection over the State's broad surface, and point out the regions where oft-times unsuspected dangers lie; and nothing but the investigations of just such a company of searchers as this recommendation contemplates can protect the State from a constant menace from dangers of this insidious character that are latent in these neglected spots.

It will not answer to tell us that these matters can be safely intrusted to the supervision of the already existing boards of health; for, as has been already stated, there are courses, that are not likely to cease to be operative, that will always continue to interfere with and to interrupt their work.

Our competence or our abilities may not be called in question, but the conditions in which we labor are neither conducive nor favorable to results that are so much to be desired. We have had all the opportunity that is likely ever to be given us, and we have been wrestling with the problem for years, to the best of our ability; but these dangers continue, and the work is not effectually performed.

Whenever epidemics break out, we are ready to investigate and prompt to take such action as the nature of the case requires. But the mischief is done then. The remedy has come too late. Perfection does not abide upon this planet; and perhaps no system can be devised that shall be absolutely perfect in its operation or that shall detect danger in every instance with unerring precision before its poison shall be diffused around.

But some of the difficulties with our imperfect methods may be clearly seen. In the first place, one great difficulty seems to be that boards of health generally, throughout the State, except perhaps in some of the larger cities, are very inadequately compensated, if at all, for the performance of such duties as even now are required at their

hands. Most of them depend upon other resources for their livelihood; and, if they are obliged to depend upon any other vocation or calling for their living, their time must, in the nature of things, be devoted to that vocation, and it becomes simply impossible for them to give the time that is needed to the interests of the State or to take the time from their daily labors that is necessary to make this department of State service as thorough as safety requires.

The usual course probably in most instances is to wait for complaints to be presented before they are investigated. Boards do not feel it to be incumbent upon them, as a general practice, to go out on tours of investigation to see what can be found. With the small appropriations made for their salaries, it is impossible for them to give the time to it that such a practice would require. Yet nothing else can accomplish the purpose for which boards of health are chosen. They are not to be charged with inefficiency or with intentional neglect of duty. But the work is more than they can do, as things are at present conducted.

When complaints are brought in to them, they act promptly, efficiently, and well. The results are seen in the diminished death-rates of former years. But it should be easier to prevent the occurrence of these epidemics than to restrict them in their course.

Another difficulty that interferes with the action of boards of health is that in the nature of things they cannot be quite independent in their acts. Whichever way a question is decided or whatever action determined on, when a complaint comes in, one party or the other is pretty sure to feel aggrieved at the way it has been settled, and so becomes an enemy,— probably for life. It is not pleasant to make enemies.

No one desires to make them needlessly; and the temptation to pass over some things lightly that would possibly be more severely dealt with if the effect of such action upon one's annual income, or his standing in the good opinion of his friend, or the alienation of his friendship, were not constantly in mind, cannot be entirely forgotten; and it must be admitted, perhaps with humiliation, that it cannot be easily overcome. Perfect independence, and therefore and consequently perfect service, cannot be secured in this way.

For these and similar reasons which are apparent, but which the

time of the Association must not now be taken to enlarge upon more fully, it seems reasonable to believe that a commission such as has been suggested by our President, that shall be paid a compensatory salary, and that shall be required to devote their whole time to the service of the State, and whose sole business it shall be, not to undertake or interfere with the work of our present boards of health, but to seek out by independent means and recommend for action such cases as might otherwise be long delayed before they would come to light, but which are a constant menace and danger to the health and lives of the whole community where they abound, would be a greater security and protection to the people of the whole Commonwealth than anything in this direction or contemplating this achievement that has ever been devised.

The advantages to be derived from such a commission would be many and great. Its independence, at least, would be assured. Its salary would not depend, as in too many instances now, upon its keeping the peace with quarrelsome or pugnacious neighborhoods; and the time would be at its disposal to give its attention wherever and whenever it should be required.

The chief objection to it, it would seem, must be the expense attending its support. It is not easy to see what else can be urged against it, and whatever there may be is left for its opponents to discover. It obstructs no machinery in the operation of local laws. It usurps no power. It disarranges no plan, it compromises no one's honor or dignity; but it should be of the greatest assistance wherever danger is lurking and wherever the germs of disease may be found. And there is every reason for believing that the people of the State will cheerfully and promptly respond when called upon to sustain this measure by unhesitatingly appropriating the necessary funds to carry this project into execution.

And now, if it is the proper thing to do at this stage of our meeting, this paper will be brought to its conclusion by recommending that a committee to consist of such a number as may be thought desirable, of which the President of this Association shall be chairman, be appointed with full power, and with instructions to present this matter to the coming legislature in whatever manner shall seem to them best and most likely to secure its immediate enactment into a law of the State.

Professor W. T. Sedgwick.— I cannot help rising to express my admiration of the point of view of the last speaker. I think that we in Massachusetts are now sufficiently one people to be ready for a somewhat more centralized sanitary bureau of information. The time does not allow any extended remarks on this subject; but I have thought for a long time, especially during the last summer when on a visit to European countries I gave considerable study to European methods of sanitation, that the point of view taken by the last speaker is sound. I should like to second his motion that such a committee be appointed. I do not remember whether he stated the number of persons. I should say a committee of five, to be appointed by the Chairman.

The motion was put to vote, and carried unanimously.

The President.— The matter is of so much importance that, with the permission of the Association, I will announce the committee later. We have had to hurry through, in a somewhat unseemly way, possibly, the list of papers; but there are certain things which some of us are willing to stay here until midnight to discuss. I certainly am. We will now go back in the list of papers for such discussion as the members of the Association now present are prepared to make on these various topics. I would like to ask if there is anything to be said with regard to the paper presented by Dr. Mason. It was a very intelligible and very interesting account of a very successful process used in one city in the Commonwealth. When it appears in the bulletin of this Association, it will be read with a great deal of interest and instruction. If there is nothing to be said on that subject, I am sure there is something to be said upon the matter of glanders. I understand that Fitchburg has had an unfortunate experience in that matter, and I think Dr. Thompson can say something of interest to us. [Applause.]

Dr. Thompson.— Mr. Chairman and gentlemen of the Association, I thank you for the honor conferred upon me in asking me to say a few words in regard to the subject of glanders as it occurs in the human subject. Owing to the lack of time, I will speak briefly.

It has been my fortune — or misfortune — to have had the care of two cases of glanders occurring in the human subject. The disease occurred in the case of a father and son, aged fifty-three and sixteen years, respectively. The source of infection was a horse that died of glanders on the 6th of July of the present year. The horse was at the State Encampment,— the first or second brigade, I have forgotten which,— and came home sick on the 30th of June. This father and son took care of the horse the following week, and the horse died the 6th of July. The 8th of July I was called to see the son. He was evidently suffering from some serious disease. At the same time I was asked to see the father. The son complained of pain in his right side and of fever, and had a furred tongue. I did not know at this time that the horse they took care of had died of glanders. I knew the horse had been sick and had died, but was told that the horse died of some lung trouble, and that there was nothing serious the matter with him. My first thought with regard to the son was that he was suffering from pleurisy, and accordingly I prescribed for him. The son was in bed; but the father was about the house, and said perhaps I had better look him over, as he was not feeling very well. He complained simply of general malaise and of headache and pain in the back. He said he had been to Fall River the previous week, and that perhaps his business cares there had made him tired, and that there was nothing special the matter with him except that. I prescribed something for him, and he went out the next day and the following day. I saw the son on those two days; but, the father being away, of course I did not see him. The next day I found the father in bed, and he was evidently suffering from some serious disease. He had a temperature of 102 degrees; and, on looking him over, I found that his spleen was enlarged, and I thought I discovered a few papillæ and small hemorrhagic spots beneath the skin. I thought in his case that he might probably be suffering from typhoid fever; and in a day or two I had the vidal test taken, but that proved negative. In the mean time it came to my knowledge, indirectly, that the horse that died had died of glanders. Then I naturally thought that there might be some connection between the two; and, to make a long story short, that proved to be the case. The most striking features about the father's case at this time were the

condition of his nervous system, a typhoid condition, and his temperature, which was over 102 degrees, and was increasing steadily.

In view of these general symptoms, having, as I have said, made up my mind that there was no typhoid fever, from the symptoms which further developed I soon decided that it was a case of glanders. Abscesses developed in the muscular tissue, the so-called nodes or farcy buds, and septic symptoms developed more and more. Pustules began to appear in various parts of the body, and developed very rapidly. Another thing that appeared was arthritis about the left knee joint. He went on from bad to worse, and died the fourteenth day. I left the care of the son at the third day with the symptoms of pleurisy, as I thought. He soon began to develop trouble about his right elbow joint; and, to make a long story short, he developed symptoms similar to the father's, and finally had pustules. The only difference between the case of the father and that of the son was that the son seemed to have more resisting power to the infection. The poison developed more slowly in his case. He was taken sick the 8th of July, and lived until the 31st. Toward the last of his disease he became delirious, his temperature increased to 105 degrees, and he finally died, as such cases very surely terminate. We were fortunate enough to obtain a *post mortem* in the case of the father, who died first; and Dr. McGrath, of the Harvard Medical School, came up and made the autopsy. I have not yet received the full pathological and bacteriological reports; but they will be made, and will be published at some future time. In the case of the father the *post mortem* disclosed nodes and abscesses about the lungs, nodes and abscesses in the muscular and cellular tissue; and he also had a septic joint — so-called septic arthritis of the left-knee joint — and also periostitis of some bones. One thing that was remarkable in his case was with regard to his respiration. He had no discharge from his nostrils at all. He had a rapid respiration, and seemed to have an unusual dryness about his nasal fossæ and about his throat and nose. At the autopsy there were lesions found at the upper part of the fossæ of an ulcerated and hemorrhagic nature, and nodes and abscesses in the lungs and an enlarged spleen. The other organs were comparatively healthy.

One interesting point which I recall was that the culture taken

from the blood of the heart, and also those from the abscesses on the lungs and from the abscesses in the extremities, showed *bacilli mallei.* I think one of the most striking features of these cases was the serious nature of the disease from the first. It was evident that some serious sepsis was going on in both. The temperature rapidly rose, and kept at a high point until death. Another striking thing was the utter inefficacy of any treatment. The treatment that I instituted was in the main supporting and stimulating. At the suggestion of others who saw the case, we tried for two days the mallein treatment,— the injection of mallein. There was no reaction from injection of the usual dose, and I tried it but two days. There seemed to be no good reason, either, for continuing it, so far as I could judge from what I learned of other cases.

These certainly were interesting cases, and I am sorry the time does not allow me to go into more extended description of them. I am sorry I have not been able to give you a more accurate and careful description. As I said before, at some future time I shall report the cases; and the pathological and bacteriological data that are with them will also be very full, from the autopsy which was very carefully made by Dr. McGrath, and I am sure they will be very interesting to all. Any who would like copies of the reports can be furnished with them after they are published. [Applause.]

The President.— Is there anything more to be said upon these cases?

Dr. Gage.— I would like to ask some bacteriologist present if there is any early method of diagnosis in a case of this kind, before the more marked characters develop, by an examination of the blood or otherwise.

Dr. Thompson.— It seems to me, from what I know of these cases, that there may be a chance from examination of the blood and possibly from examination of the urine. It is said the *bacillus mallei* is present in the urine and in all exudates. It is a fact that the bacillus was discovered in the blood in one of these cases. Another thing, it seems to me, from the symptoms observed in these cases, that the infection is carried and must be carried through the blood rather than in any other way on account of the rapidity with which

the symptoms develop, and the universality, so to speak, of the symptoms. These pustules will develop in twenty-four hours. You will have a very small papule somewhere on the skin, and at the end of twenty-four hours it will develop into a full pustule. Some of these pustules have considerable injection around the base, and many of them remind you of the vesicles and pustules in small-pox.

Dr. Mason.— I examined a specimen of the blood from the father to test it for typhoid fever, as stated by Dr. Thompson, which proved negative. At that time, however, the doctor knew nothing about the glanders infection, so there was no examination made in that line. But I do not think, in reply to Dr. Gage's question, that there is any way that you can tell — none that I know of, at least,— by the blood. My idea is that there are bacilli in the blood, but they are very infrequent early in the disease. If a bacillus stops or lodges in a certain place, it very quickly becomes a focus. The bacilli propagate so rapidly that one or two lodging in one place will produce a node or postule probably within the time that Dr. Thompson states,— twenty-four hours. But I think they are so infrequent that it would be a very difficult task, indeed, if not impossible, to isolate them from the blood in the early stage of the disease.

The President.— In his welcoming remarks here, his Honor, the Acting Mayor, called our attention to some local conditions in Fitchburg that interest Fitchburg very much and that are of a sort that interest every town and every city in the Commonwealth. I should be very glad, and I think the Association as a whole would be very glad, to hear something more about it. I am told that Professor Sedgwick knows more about it than almost any other man. He generally does, about these matters ; and I think we should all be glad to hear from Professor Sedgwick. [Applause.]

Professor Sedgwick.— The President gives me a very damaging reputation.

The President.— Not at all.

Professor Sedgwick.— It is usually the case that the man who knows it all knows nothing. But in this particular case I want to say a word as spokesman for the Sewage Commission, and the

Board of Health, and the citizens generally of Fitchburg. Fifty years ago the river which you have seen flowing through Fitchburg was a clear, bright, agreeable-looking stream; but fifty years ago Fitchburg hardly existed. In 1885 Fitchburg contained about 15,000 people, and had got to the point of polluting its river very seriously, indeed. By the last census there are 30,000 people in the city. The population has doubled in the last fifteen years, and from the little town of fifty years ago Fitchburg has become one of the proud cities of the Commonwealth. The citizens of Fitchburg have long looked upon their river with mingled feelings,— respect, because it is the source of the town's growth and wealth; affection, for what it was; and a certain amount of fear, for what it was becoming. They have, at various times, talked of doing something about it, and have always intended to make it right; but these things, as you know, even in Massachusetts cities, go slowly. The unsanitary conditions that Dr. Everett dwelt upon come upon us not suddenly. If they did, we should resist them; but they come little by little and almost unawares. The river gets a little worse every year, but not so much worse as to very much embarrass or frighten the people at large.

Fitchburg, however, prides herself on her situation, her fine air, her prosperity, and her energy. And the time has come when the citizens of Fitchburg are determined to clean up the river. They are determined to come into line with the most alert and active cities of the State, and to cease polluting the north branch of the Nashua, so that that branch may become as clear and, we hope, as famous as the south branch, which goes to make the great Wachusett reservoir. In 1895 the citizens of Fitchburg, through their city government, went so far as to take steps looking toward the purification of the river locally. They appropriated money, and had plans made by their able engineer, Mr. Hartwell, and the consulting engineer, Mr. Allen of Worcester, whom they employed for the building of a trunk sewer which should take out of the river in their immediate neighborhood all the sewage and wastes of the city. They did not, however, consider what they should do with the sewage after they had got it into the trunk sewer. It is fair to suppose that they thought that, if they could take it down toward Leominster and put it into the river

at that point, Fitchburg, at any rate, would be all right. [Laughter.] And so, no doubt, it would have been. But, perhaps with the feeling that that, after all, was only a part of the problem, nothing was ever done about it. And it is only within the last few months that a determined stand has been taken to purify the river within the limits of Fitchburg and then to take the waste matters, and not put them back into the river, but somehow purify them, so that the whole river in this neighborhood shall be clean and sweet. Last April a Commission was appointed, which is known as the Sewage Commission, consisting of the mayor, chairman *ex officio*, the city engineer, *ex officio*, who is still, fortunately, Mr. Hartwell, and three other eminent citizens,— Mr. Woodward, Mr. Estabrook, and Mr. Lawton. These men have the confidence of the community. They have no axes to grind,— they have only the river to clean; and they have the good reputation of Fitchburg at heart. You will notice, as you see the river, that it is very filthy and dirty. It is a menace to the well-being of the city and destructive to the well-being of the river below. I hope that before the next meeting of this Association in Fitchburg — and may that not be a very long time — the city will have taken steps toward the proper purification of its sewage. This, however, is a matter for careful investigation and inquiry. And the Sewage Commission intends, and is authorized by the city government, to employ expert aid and counsel, such as a distinguished sanitary engineer, to prepare proper plans for the right purification of the river. Fortunately, there is in Leominster, just over the line from Fitchburg, a lot of comparatively waste land, which on the face of it seems to be very well adapted to sewage purification. And, if this city of 30,000 inhabitants shall first build an intercepting or trunk sewer which shall carry the wastes away from the river within the city itself and then purify them on this waste land and return the water to the river below pure and sweet, and shall do it in a scientific and economical manner, another leaf will have been added to that crown of laurel which Massachusetts already wears for her scientific method of dealing with these problems.

Massachusetts is a State of cities, and is already looked up to all over the world for some of the best experiments that have ever been made in sewage purification. Fitchburg has the opportunity to

profit by the experience that has gone before. It is her intention and her strong resolution, so far as I know it, to carry out the best plan for her sewerage in the right way. I think that this Association ought to know that one of the principal cities of the Commonwealth is determined to cease polluting the river within its borders, and to install here a system of sewage purification thoroughly up-to-date, of the latest and best type, economically but thoroughly built, and in every way creditable both to the city and the Commonwealth. The Board of Health has been helpful in every way. The citizens are willing and ready, I think, to do the right thing. It seems to me a matter of general interest to this Association, and especially of local and timely interest on this occasion, to know that this beautiful and progressive city whose guests we are to-day is about to embark upon a sanitary scheme of so much local importance and general significance. [Applause.]

The President.— I hope the Association may hear something more about this. It certainly is a most interesting matter. I do not wish to depreciate in any way the south branch of the Nashua; but I would only like to say, as a mere matter of justice, that, if the Metropolitan Water Board had taken the south branch of the Nashua below the thriving town of Clinton, we should not have been quite so proud of it. I hope we shall hear something more about this Fitchburg experience.

Mr. Woodward.— I think the professor, in his statement of the case, has covered the ground thoroughly and ably; and I know that no one could have stated it better. All I can say is that those of us who live in Fitchburg and are interested in this project are going to do the very best we can. If we can learn what the proper course is, we shall certainly pursue it; and we think we have some very able men to whom to appeal. The Massachusetts Board of Health will give us, I presume, all the information it can; and we shall ask of the cities of Massachusetts all the advice which they can give. We have already been to Worcester, and received very valuable suggestions from that city. The gentleman from Worcester at my right says he hopes we won't be at the expense in experimenting, and will

avoid some of the mistakes his city has made. I hope we may. It seems to me in the year 1900 we ought to be able to start from the very first and work directly toward some desirable end, so that we shall know about what we are going to do when we get our works in operation. The question, I know, is new, and is in the experimental stage; yet I believe enough has been accomplished, so that we can go forward without great mistakes. [Applause.]

Mr. Coffey.— I just want to say a word. I said to the gentleman who has just spoken that, *if* any mistakes had been made at Worcester, I hoped they would be able to avoid them. [Laughter.] I did not admit that any mistakes had been made. [Renewed laughter.] However, Worcester has spent a great many dollars on her sewage purification scheme; and, certainly, the money that has been spent there in perfecting that scheme of sewage purification is for the benefit of other cities in the State. We have always contended in Worcester that the State ought to reimburse us for the sum of money that we expended there, because we have been educating the rest of the State. [Laughter.] Our system of sewage purification has cost us into the millions, and I certainly hope that Fitchburg will not find it so expensive as Worcester has. We are now engaged in doubling our sewer system; that is, formerly the combined system was in force. The storm water and the sewage proper went altogether into the Blackstone River,— a river in name only. It was, after all, only a small stream; and I imagine your river here in Fitchburg is a good deal like it in that respect. Now we have been forced by the necessities of the case, and by the Massachusetts legislature [laughter], to build a new system of sewers, taking the storm water out of the sewage proper; and that has cost us not only a good deal of money, but a good deal of inconvenience. The streets are torn up all over the city by the building of this new system of sewers. The houses will all have to be detached. Where formerly the rain-water leaders all emptied into the regular sewer system, they have now all got to be detached, and run into this sewer which will take only the surface water. The system of purification is a chemical system,— chemical precipitation; and lately we have supplemented that by adding

a filtration scheme, because the towns below us have complained that the sewage was not sufficiently purified or the effluent that went into the stream was not sufficiently purified, and that, consequently, the nuisance still existed. The legislature, and, I think, the Supreme Court, has insisted that Worcester should go to still greater expense, in order to more fully purify its sewage. There is no question but what it was a nuisance below Worcester. There is no doubt about that. No man in Worcester will deny that. The only thing that we in Worcester claim is that we were allowed by the legislature originally to use that stream as a sewer. After we had so used it, and Worcester had grown to be one of the great cities of the country,—as you know by the last census, we are growing very rapidly there, having now 118,000 population,—its sewage was turned into this small stream, and made it very foul and very much polluted. But I only got up to say a word in explanation, because the gentleman on my left did not clearly understand what I said. I am not willing to admit that Worcester has made any mistakes. [Laughter and applause.]

Professor Sedgwick.—I want to say a word about Dr. Everett's paper. I believe that the time has come for Massachusetts to take another forward step. In this family of Massachusetts people we may venture to say what perhaps we would not say publicly outside; namely, that Massachusetts has led in sanitary matters in this country for a number of years, perhaps always. That is, it has led the other States, it has led the rest of the United States, and in some respects it has led the world. But in certain other respects, although still perhaps ahead of other States of this Union, it is lamentably behind the times. The matter which Dr. Everett has touched upon I regard as a vital one. It was said in England and on the Continent,—particularly in England, which has always led the world in sanitary matters,—some years ago that they never would get ahead any until they had two things: first, a somewhat more centralized sanitary administration; and, second, medical men more highly educated on the sanitary side. Those ideas were carried out. The sanitary administration was made more centralized and the universities began to train men, and give them the

degree of Doctor of Public Health. I believe that eventually we shall have both of those things; and I want to see Massachusetts lead,— not necessarily exactly along these lines; for precisely as our cities and our universities and our technical schools differ from those of Europe, and always must differ from them, so our sanitary systems and our systems of medical education are bound to differ from those of Europe. But the ends to be reached are very much the same in both cases. We want, for one thing, to relieve the local boards of some of the pressure of local influence. Secondly, we want to make permanency of tenure in sanitary matters much more secure than it is to-day. Why is it that the faces of the members of this Association change as they do? It is not death that does it. It is the system of rotation. A man just gets well experienced in the board-of-health work, when out he goes; and another man, who knows little or nothing about it, comes in. That has its advantages. It educates a lot of the members of a community in sanitary matters up to a certain point; but when, as was the case this summer, I find a man in an English town who has been the medical officer of health in all that district for twenty-five or thirty years, who knows all the ins and outs of all that region, who can put his finger right on the map and show you just where the bad places are, and where they are doing good work, who is secure in his position and has his profession in doing that work as a medical man, why, you can see — any one can see — that there is a better administration going to come from it. We want less local pressure on boards of health. We want greater permanency of tenure among the officers of boards of health, and then we want medical men more highly trained along these lines. Of course, the universities, as a rule, are not going to prepare men until there is something for them to do. You cannot expect men to go and fit themselves to be medical officers of health, and know all the ins and outs of sanitary science as well as of medicine, and then have nothing to do.

Now it is a great problem, of course; and I know that local influences are at work,— this, that and the other. No matter. Massachusetts is a State of cities. It is a State of marked intelligence, and it has got to lead the country in these things as well as in other things; and somehow or other — either in these ways or in similar

ways, or in some other ways — we have got to have those things that I have mentioned before we shall have sanitary administration arranged as it ought to be in a great modern State. And the art is so long nowadays that it is not possible for a busy practitioner to turn in and make the ideal board-of-health man, — I mean in respect to administration, the man who shall suddenly become the executive officer of a board of health. That position needs special training, and is going to need it more and more all the time. I want to see this State lead, and I want to see this Association lead. I believe this Association has breadth enough and intelligence enough to go before the legislature, and say, "We want some small changes looking in this direction, something which before long shall lead us up into a more perfect administration." We have got great things still to contend with. Our typhoid fever deaths are far larger than they ought to be, even in our State,— far larger than in most well-regulated States. Our general death-rates are a good deal higher than they ought to be. In Oxford, where I spent some time this summer studying sanitary administration, the death-rate fifteen or twenty years ago was as high as it is to-day in Boston. To-day the general death-rate is only $14\frac{1}{2}$, and it has been going steadily down since they got better administration. They had no typhoid deaths at all in that city of some 50,000 people all last year. It is wonderful the way in which they stamp out anything of that kind that comes in, after following it up sharply. They are not satisfied with general investigation. They are not busy physicians who have to steal time to do these things; but their health officials are men appointed to do those things and do nothing else, or very little else. The result is there is specialization there, there is good service. We have got to come to something of that kind, or our typhoid deaths will continue to be a reproach to us, as they have been for many years and are to-day; and so with many other diseases.

I think Dr. Everett has done a commendable thing for the Association in bringing this matter up. There are places in this State that need to be more carefully looked into. We don't want to rest on our oars: we need to go ahead. I want to see this Association, and especially this State, go ahead and lead the country, as it has long led it, in sanitary matters. [Applause.]

Mr. Coffey.— I want to move that the Association extend a vote of thanks to the Fitchburg Board of Health, and through them to the city government of Fitchburg for their generous hospitality shown to the Association to-day.

The President.— You have heard Mr. Coffey's motion, which every man outside of Fitchburg seconds; and I will put it to a rising vote.

The motion was put, and the assembly rose to its feet. The meeting then adjourned.